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SPACE SCIENCE: PAST, PRESENT AND FUTURE

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HEARINGS

BEFORE THE

SUBCOMMITTEE ON
SPACE SCIENCE AND APPLICATIONS

OF THE

COMMITTEE ON
SCIENCE AND TECHNOLOGY
U.S. HOUSE OF REPRESENTATIVES

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FIRST SESSION

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SPACE SCIENCE: PAST, PRESENT AND FUTURE

TUESDAY, OCTOBER 8, 1985

House of Representatives,
Committee on Science and Technology,
Subcommittee on Space Science and Applications,
Washington, DC.

The subcommittee met, pursuant to notice, at 1:54 p.m., in room 2325, Rayburn House Office Building, Hon. Bill Nelson (chairman of the subcommittee) presiding.

Mr. **Nelson**. Good afternoon.

I'm going to dispense with the opening statement at this point so that we can go ahead and have the testimony from Dr. Sagan, and we will honor your time commitment, 2:15.

So why don't you proceed?

STATEMENT OF DR. CARL SAGAN, CORNELL UNIVERSITY, ITHACA, NY

Dr. **Sagan**. Thank you very much, Congressman Nelson.

I will try to keep my remarks brief because I understood that it is the question period that you had mainly in mind for this session.

I'm very pleased that Congress, in its various incarnations, is looking at the nature and future of space science. It's a very major, very important part of the U.S. space program that tends to be given short shrift, as the selection of pictures in this room clearly indicates—not one picture that is unconnected with the manned space program.

And, yet, the accomplishments of the unmanned program, unpeopled program, has been extraordinary, examining every planet from Mercury to Saturn, and soon to Uranus. There's only one moment when we look for the first time at all those other worlds, and that is in our generation. We are at an absolutely unique moment in the history of the human species to make that bridge from not knowing the planets to knowing them for the first time, and to see how understanding them helps us understand our own planet. And there are many examples of how knowing another world helps us in knowing our own.

I would like briefly to lay out an idea, a suggestion, that many of us think is promising for the long-term goal of the American space program.

NASA has a history of developing extraordinary technological capabilities with sometimes not the foggiest idea of what it's going to do with them. A well-planned program would have a goal established first and then you would see what kind of technological capabilities you need to accomplish it. But very often—in fact, almost always—it goes just the other way around. As a result, except for the Apollo era, NASA has simply lived from fiscal year to fiscal year with very little in the way of long-term planning.

If you take a look at the constituencies that support NASA, you find there is, of course, the aerospace industry, a technological constituency; there is a scientific constituency which you are largely hearing from today; there is a manned space flight constituency, which is very powerful. Again, I point to the pictures on the wall. There is a public constituency which has many interests, but one of which is exploration—the wish, and it is a very deep human wish; it's been with us as long as we have been on this planet—the wish to go over the next hill to see what's beyond. And that kind of exploratory curiosity is in many respects responsible for our success as a species.

Well, this is just the moment that we have bound up the Earth here and are able fully to explore our world, and it's just the most natural extension of what we have been doing to seek out other worlds.

If you put these constituencies together, you can imagine a range of possible future goals for NASA, but if manned space flight is involved, they are, of course, expensive goals. Manned missions generally cost 10 to as much as 100 times as much as comparable unmanned missions to the same object by robot vehicles when the objectives are scientific and exploratory.

And so, as with Apollo, a major manned mission, a mission which combines the various constituencies I've mentioned, must have some political or at least historical purpose which justifies the larger expenditures of funds. I would like to suggest one such mission.

In July of this year a meeting was held in Washington cosponsored by the Planetary Society—it's a hundred thousand member organization, the largest space interest group in the world—and the American Institute of Aeronautics and Astronautics, the largest organization of American aerospace engineering talent. It was held at the National Academy of Sciences and was devoted to two things:

One, it did a retrospective of the Apollo-Soyuz mission 10 years earlier. The three Apollo astronauts and the two Soyuz cosmonauts were there, and it was a remarkable reminder of what the United States and the Soviet Union can do together if they so wish.

But the main focus of the symposium was to look forward to missions to Mars. Mars is an extraordinary place, a world of wonders. It's a planet which today is bone dry, but a billion years ago liquid water apparently was gushing all over the planet. The planet's covered with ancient river valleys—an interesting reminder, incidentally, that major climatic change can happen on an Earth-like planet.

It is a place with soaring volcanic eminences, the largest of which is almost three times the height of Mount Everest. There is a set of extraordinary stacked plates in the polar regions, suggesting again periodic climate change. In many places on the planet there are hints of stratification, suggesting a geological record which might be there for our decrypting, decoding, if we were down there.

It's a place with major dust storms sometimes of planetary dimensions, and, in fact, it was the Mariner IX investigation of one such dust storm in 1971 which paved the way for the discovery of nuclear winter on the Earth, a reminder of the extremely and unexpectedly practical applications that can follow from studying some other planet that seems to have no practical benefit whatever.

To go to Mars with humans purely for scientific reasons I say cannot be justified financially. It's simply too expensive. How expensive? A study made by Science Applications, Inc., for the Planetary Society suggested it can be done at a little less than the cost of the Apollo missions. Now that's a substantial amount of money. It is, of course, trivial compared to the cost of other kinds of space technology that we apparently take seriously, such as the so-called strategic defense initiative, but it's a very large amount of money compared to what NASA is used to, especially these days.

Now imagine the following circumstance: Imagine—and I'm speaking only for myself—imagine that the United States and the Soviet Union came to their senses one day and wished to do something cooperatively, something major, something that would capture the imagination of the planet, something that would be done on behalf of the human species as opposed to many of their other collective enterprises which work in the opposite direction. Then a joint manned mission—I hate the word manned; men and women—humaned, personed, crewed mission to Mars would make sense.

Imagine the construction with the equivalent of one or two dozen shuttle payloads of an interplanetary transfer spacecraft in Earth orbit—of course, every construction step there on the evening news. United States and Soviet welders or equivalent working together. Then the spacecraft is finished; then on its way to Mars in, I think, extraordinarily apt metaphor of the condition down here—the United States astronauts would have their lives dependent on the Soviet cosmonauts doing right and vice versa.

Then they get to Mars; land on the surface. Sometimes it's said, "Isn't there a big problem about who will step out first?" My answer is to tie the ankles of the Soviet and American commanders together and have them hop out. It's only 38 percent of Earth gravity, so they won't break their ankles. I don't think priority will be a serious concern in such a mission.

Then imagine some period of time, maybe months, of joint exploration of Mars and its wonders. Imagine a roving vehicle going down an ancient river valley for tens of kilometers. There's no telling what could be found. What can be guaranteed is that Mars will not disappoint; it never has, with the possible exception of the Viking search for microbial life.

Then, samples in hand, the crews lift off, return back to Earth, and every step, as I say, including the time spent on Mars available daily back on the evening news on Earth.

I maintain that such a mission—I think there are many of us who have the same view—would be a powerful token of the United States and the Soviet Union wishing to turn around the present quarrel which has by now threatened everyone on this planet.

It is clear that there is a significant body of support for such an enterprise which cuts across ideological and political and national boundaries. For example, Jack Schmidt and I, who have very different political views, keep saying almost word for word what the other is saying on this issue. Joint United States-Soviet Mars exploration has an extremely broad and ecumenical support.

This is by no means the only possible major long-term space goal for the United States, but it appears to be the most promising one. You could imagine bases on the Moon or missions to Earth approaching asteroids, but in terms of major scientific returns, of capturing the world's imagination and doing something sufficiently challenging, a joint mission to Mars seems to me hard to beat.

Thank you.

Mr. NELSON. Thank you. Your comments are most timely, for this very subject will be discussed starting next Monday in Moscow with our host, the Academy of Sciences. And so I'm most appreciative of your suggestions.

What do you think about an interim step? What do you think we ought to do in the way of Soviet cooperation in space as an interim?

Dr. SAGAN. Well, the sort of mission that I've been describing almost certainly could not be mustered before the turn of the century. There are 15 years between now and then, and clearly it would be a good idea to start in a smaller, more modest way.

There are lots of things that can be done, some useful, if unimaginative—like joint search and rescue agreements which aren't a very big deal; to cooperative planning of independent missions to the same object. For example, the Soviets have a very ambitious mission to Mars scheduled to go later this decade. The principal focus appears to be on Phobos and Deimos, the two moons of Mars. The United States has a more modest geochemical and climatology explorer spacecraft, but they'll be looking at the same objects with similar instruments. It would be useful at least to do some cross-calibration.

Likewise, there will be a Venus orbital radar by the United States, and perhaps—I think quite likely—the Soviet Venus missions to follow up on their long and successful series of Venera spacecraft. There, again, cross-calibration of instruments, exchange of data, that sort of thing.

This has already been happening informally in the scientific community. This administration has been reluctant, in part—but only in part—on grounds of technology transfer, to encourage such exchanges. But because the Soviets are so capable along these lines, the technology transfer is at least a dead heat and some competent testimony before Congress has held the United States gains more than it loses from the Soviets in such exchanges.

But we could imagine a precursor mission to Mars, one that didn't involve humans but tested out the ability of the two countries to work together. Here is a possibility which was proposed informally by Soviet scientists, just to give you a feel:

Country A would launch an unmanned mission to Mars which would involve a roving vehicle which could do sample collection. It would rove on a predesignated traverse across the Martian terrain, collecting samples which would be exciting to examine close up. After a long traverse, maybe as much as a year, it would gather all of this material in a place which is very easy to land on, and that's the end of the mission of country A.

Country B then lands at that landing site. It has very limited mobility, but it can pick up this cache of material that's been sequestered there and then carry it back to Earth.

Now the beauty of this scheme is, if country A succeeds but country B blows its spacecraft up on the launch pad, then the samples are still there for a subsequent mission. Country A's work has not been wasted. If country A's rover gets bogged down in the sand dunes and can't get to the safe place where country B is to land, country B's lander can, nevertheless, go out a little to gather some samples, not as diverse a range of samples but still some samples, and return it to the Earth.

So this is a kind of mission design in which neither country is entirely dependent on the success of the other or on the maintenance of the cooperative frame of mind in Washington and Moscow.

I think that's a very good model, not necessarily this particular mix of missions, but a very good model of how to do such joint missions in the early stages when the two countries are feeling each other out.

Mr. Nelson. What year do you have in mind for that mission?

Dr. Sagan. We could have done something like this in the immediate post-Viking days. It is not well beyond our capability. It's pretty easy to do. I think there is a particular year which for symbolic reasons, and also in terms of the development time scale, makes a great deal of sense, and that's 1992, for this precursor mission—1992 being, of course, the 500th anniversary of the discovery of America by white people and the 75th anniversary of the Bolshevik revolution. Both countries might find some significance to 1992.

Mr. Nelson. And the 35th anniversary of sputnik.

Dr. Sagan. I'd forgotten that.

Mr. Nelson. Senator Matsunaga and I have filed resolutions in each House calling for 1992 to be the International Space Year, and under that umbrella of those anniversaries that's a good time in which you could get nations of the world together again under a common project.

Well, I appreciate your comments.

Before I turn to my colleague, let me ask you: Do you think that the National Commission on Space is moving in the right direction with regard to space science?

Dr. Sagan. Well, I don't know what their conclusions will be. It's too early to say. But in terms of the membership and the chairmanship of that committee, it is a very distinguished group with a very wide range of capabilities and interests, and certainly at least a few scientists on it, one of whom, George Field, is in the audience here today. I think we should pay a lot of attention to what they have to say.

One other remark I forgot to make: I was at a meeting last November, almost a year ago, in Bellagio, Italy, a very interesting meeting of world religious leaders and scientists. Roald Sagdeyev, the head of the Institute for Cosmic Research of the Soviet Academy, was there, and we started talking up, just for discussion, such joint missions.

The religious leaders, their concerns were initially down here, although there is a certain religious preoccupation with up there as well. But as we talked about United States/Soviet joint missions to Mars, they got very excited. It was very interesting to see, across the spectrum, representatives from the World Muslim Congress and various religious groups in the United States and other nations get caught up by the idea of a major mission done on behalf of the human species.

This is only anecdotal, but I have a very strong sense that this is something that could really capture the imagination of the planet, to the benefit of all participating nations. It need not be restricted, of course, solely to the United States and the Soviet Union.

Mr. Nelson. I happen to share your enthusiasm on this subject. I also might point out that Tom Paine and his executive director, Marsha Smith, are going with us on the trip to the Soviet Union.

Mrs. Meyers.

Mrs. Meyers. When I talk to people at home, I say that there are several issues that are probably major issues for the rest of the decade and probably for the rest of the century really, and they're the usual ones—trade and the deficit and defense and arms negotiations generally. But certainly one of them is our relationship with Central and South America and our neighbors.

And at—I talked about this at one place the last time I was home, and a gentleman came up to me afterward and said that he thought maybe one of the best ways that we could hold out our hand to our neighbors to the south and involve them in something that was exciting to all of us and restore relations was through the space program. And he said, "I just can't imagine why we haven't done this in the past. Why don't we involve someone from Mexico on one of the space flights, include them in some of the marvelous and exciting things that we are doing?"

Sometimes I think we think always about how to hold out our hands to the Soviets, which I think is a wonderful idea, too, but have you—would you comment on that? How can we involve our neighbors more in the space program?

Dr. Sagan. I would think that, if we were serious about it, we would not restrict ourselves to cosmetic gestures such as inviting a Mexican astronaut aboard a mission, but would attempt to build up the technological infrastructure in those countries so that they could make genuine scientific and technological contributions.

There are excellent scientists in Mexico. I know the Mexican astronomical community, and scientists in other Central and South American nations, but they have serious deficiencies in the educational opportunities available to them, and in the technology that's available. If the United States were serious, we could, of course, do a great deal, and those improvements in technology would have other consequences, not just involving other nations in the space program, but developing their general technological expertise for their own economic benefit.

Mrs. Meyers. Thank you for those comments.

Mr. Nelson. Mr. Smith.

Mr. Smith. No questions.

Mr. Nelson. All right. Dr. Sagan, thank you for being with us. And we have honored your commitment. We're late by 2 minutes.

Dr. **Sagan.** Thank you for your indulgence, Congressman.

Mr. **Nelson.** Yes, indeed. Have a good day.

Dr. Hinners, before we take your testimony, let me sort of set the stage here with some of the comments that I wanted to make.

We're looking at these 3 days of hearings to review past major accomplishments and to give us a perspective as we look into the future. Space and Earth sciences are recognized by this committee as certainly a major contribution to our society's well-being. And as part of this subcommittee's oversight process, the—concurrently the Congressional Space Caucus is also holding hearings to focus attention in this area.

In the early days of space science we studied space. We discovered the phenomena that is discovered and named after one of our witnesses today, the Van Allen radiation belts. We learned about the near-Earth environment. And through some rather astonishing leaps, we now see space as a place from which we can study other things. We're using space for meteorology, oceanography, biology, astronomy, and now even medical sciences.

These are exciting times. We're going to be seeing in our lifetime the finding of answers to a lot of the questions that have been in the very fabric of man's intellectual curiosity. So it's a privilege for us, as this committee, to be a part of this exciting scenario.

[The prepared opening statement of Mr. Nelson follows:]

OPENING STATEMENT OF HON. BILL NELSON

Today, the Subcommittee on Space Science and Applications begins a series of hearings on a subject of immense importance not only to Congress as an oversight body but to our society as a whole. Over the next three days we will take a very close look at the space and earth sciences. We will review some of the past major accomplishment and the contributions they have made to our perspective on our place in the universe. We will use this background to project where we should be in the future and what directions our research should take to get us there.

The space and earth sciences are clearly recognized by Congress as a major contribution to our society's well being. Coincident with these hearings, which are a part of this Subcommittee's oversight process, the Congressional Space Caucus will also be holding hearings to focus attention in this area. I have reviewed their program and I know that it is an outstanding one. I urge those of you who are able to attend those as well.

In holding these hearings we are cognizant of the fact that the space and earth sciences make up only a fraction of the NASA budget. However, this small fraction supports a much greater proportion of U.S. basic research in these disciplines. As a consequence, any adverse impacts on these budget categories may have severe reverberations throughout the scientific community.

We are also aware of the comprehensive Science Policy Study underway in this Committee. The Science Policy Task Force will focus on the issues of maintaining America's leadership in science in view of the changing environment facing us over the coming decades. In this environment, the Subcommittee will pursue those areas that relate space and earth sciences.

In the early days of space science, which were not so long ago, we studied space. We discovered such phenomena as the Van Allen radiation belts and we learned about our near-earth environment. Through some rather astonishing leaps and bounds we now see space as a place from which to study other things. We use space for meteorology, oceanography, biology, astronomy and even the medical sciences. When I have the privilege to fly on the Shuttle as a mission specialist I hope to be participating in some experiments to grow protein crystals that may have profound benefits to mankind. These are indeed exciting times. We will in our lifetimes be finding the answers to questions which have been the very fabric of man's intellectual curiosity. I am thrilled to be a part of this scenario.

In today's hearing we will receive a broad overview of the importance of the space and earth sciences from two eminent scientists—Dr. Carl Sagan of Cornell University, and Dr. Noel Hinners, Director of Goddard Space Flight Center. Then, we will focus on an area of long-term interest to the Subcommittee, solar-terrestrial physics. Sun-earth interactions have been linked to everything from the stock market to the ice ages. We will hear from Dr. Lou Lanzerotti of Bell Laboratories, Dr. Tom Krimigis (Crimejis) of Johns Hopkins University and Dr. James Van Allen of the University of Iowa.

I will now turn to the Ranking Republican, Mr. Walker, for his opening statement.

Mr. NELSON. So, with that as sort of the frame of reference as we leap out and start looking to the future, we're just delighted to have Dr. Hinners, who is the Director of the Goddard Space Flight Center.

So please proceed with your testimony. If you'd share with us just in a conversational way, instead of a written statement, we will have that written statement as a part of the record.

STATEMENT OF DR. NOEL W. HINNERS, DIRECTOR, GODDARD SPACE FLIGHT CENTER

Dr. HINNERS. Thank you, Mr. Chairman, members of the subcommittee.

I do commend you, first of all, for your desire to have us here to help you learn about what our Space Science Program is about and to see where it should be headed.

Indeed, if you look back historically over the past 25 years, they've been astounding in terms of the progress we have made technologically and scientifically. If we look at astronomy, the astronomy we do today just is not possible without the vantage point of space—to rid ourselves of the obscuring atmosphere, to get ourselves above where we can look at the high energy coming in from cosmic phenomena, quasars, black holes, things that you've heard about and which sound rather mystical, and yet are some of the most fundamental physical phenomena that we could possibly dream up.

The early days were the easy days in a sense. Discoveries occurred every time one would launch a satellite or go up and look in a new part of the spectrum. In astronomy those days are not yet over. There still remain parts of the spectrum we have not investigated. And even where we have looked, it's been a cursory look, believe it or not.

Our challenge now is to stay up above the atmosphere, to look across the spectrum, to bring our new detector technology, which each time we go enables us to see further or with higher resolution or with better spatial resolution, and we learn amazing new things. There appears to be no end as our technology keeps advancing.

One might ask occasionally, Of what use is this? And I do get the question posed. A scientist might be tempted to say knowledge for the sake of knowledge. I would take great issue with that kind of response. To me, it is much deeper than that, particularly as we look to see where society is headed today, with a major goal of educating our people, especially our youth.

We're making our youth, our people, into a very curious people, taking advantage of the epitome of evolution, if you will, at this point in the mind of man. As such, we're curious.

The basic happening, if you will, of discovery is an elating experience. It's something which was missed in part of our society. In ancient times mythology provided some of the mental gymnastics, if you will. Then there was a period in which mythology was downplayed and, as I mention in my testimony, D.H. Lawrence bemoaned the loss of mythology and how the scientist was destroying the mythology of the Sun.

I think if he were alive today and understood the scientific discoveries, he would find a return to the awe and wonder of what is happening in nature. It is so mind-boggling and so beautiful when you do see what nature has provided.

That I think is the key to why we search and why we're curious, and I think it's an aspect which we should never forget and must continue to bring to the public to share the elation and the joy of discovery with the public, who is, after all, paying for this endeavor of scientific research.

Astronomy is not the only area making such progress. Our planetary exploration—in the early sixties, of course, again, each time we would venture near a planet, a major discovery. Essentially, we had not seen them close up, to see the volcanoes of Mars, the canyons, the volcanoes of Jupiter and Io, the rings of Saturn—completely new discoveries. And in January we'll have the vantage point to see Uranus for the first time close up.

We returned after these first discoveries to look further, to look at the weather phenomena, if you will; to look at the geological processes at work on the planets. We've made great strides in understanding how planets are formed, what brought them into being.

We're at the point where we're returning some of that knowledge, and the aspect of global science, if you will, which we've gained from the planetary program, to looking at the Earth. Indeed, now we look on the Earth as one of the planets, which it genuinely is.

There's a coherent framework that's been now developed to look at our planets, understanding their solid evolution, how their atmospheres behave. The topic called "comparative planetology" sometimes is a valid one. By looking at the spectrum of planets, at the spectrum of atmospheres, we begin to understand the process, whether it's active on the Earth, on Venus, or Mars.

Anyone alone looked at appears sometimes so complex that you can't figure it out, but by looking at the spectrum with different starting positions, different end points, one can do a better job frequently of understanding how collectively they have evolved and then, in turn, how any individual one has evolved.

Obviously, we don't go to the planets just to find out about the Earth. We're learning about the origin of planets. We're combining that information now with what we're finding in astronomy. The infrared astronomy mission—wonderful new information on how planetary systems may form. So we're coming at the origin of planets from two ends: from astronomy and from our planetology.

Looking at the Earth per se, you're all quite familiar, of course, with the weather satellites. Weather satellites developed out of basic research opportunities in the early sixties.

The Landsat, looking at the Earth with multispectral sensors, evolved out of research programs. Some of those programs have now become what is called operational. It's a shame in a sense that they've taken on that characteristic, because those same programs still supply very valuable, much-needed scientific research data. And, unfortunately, that has also led to a lessening of the scientific research satellite for Earth applications in many areas.

Nevertheless, what we've seen over the last decade is that satellites are an extremely important additional tool to the earth scientist to understand crustal dynamics, for example. Using laser targets in space, we now can measure the motion of our continent down to the centimeter, possibly millimeter level—to put proof behind the theories and the evidence for continental drift, plate tectonics.

We're seeing with our research satellites that the ocean and atmosphere have a very intimate connection; that one cannot study the oceans in isolation nor study the atmosphere in isolation. The feedbacks, the interconnections are just too strong.

Satellites promise a global perspective, a time series of measurements in many areas of the spectrum, so that we can look at, say, a cloud cover at the same time as ocean temperature, feed them together to put into our models. I say models. Models of what? These are models which generally are based on computer developments, technology; models which allow us to predict the behavior of the major atmospheric or ocean systems. And, indeed, that's where we're trying to get in Earth sciences—is to a predictive capability, to understand well enough where our major systems are going so that we can take the correct political and social actions downstream.

This requires, of course, that we have the research programs that allow us to get data for the models and, in turn, update those models and improve their predictive capabilities. That is a long-term program. It will take decades to get there, and calls for a commitment to Earth science research on something much better than a mission-by-mission basis. We must have a coherent long-term strategy which is now being worked.

It's a strategy which goes beyond the space agency. NASA itself has no grip on research from satellites. There is no reason, of course, why NOAA, why the Department of the Interior, or anybody else can't launch and use a satellite to collect basic scientific information relative to their roles.

What is required is that we have a bringing together, a coordination of this activity, and agree on how we're going to make progress toward understanding Earth as a global system.

In our space physics programs, there would be no space physics per se without the satellites. Prior to the space age, one could study aurora, magnetic field variations, but it wasn't until we got into space and Jim Van Allen, you mentioned, discovered the radiation belts that we really saw the complexities of interplanetary and near-Earth space—this magnetic cocoon, if you will, around the Earth with extremely complex phemonena occurring in it—electrons coming down into the atmosphere causing the aurora, electrical currents going around the equatorial zones, solar wind sneaking in somehow, great energy release in the geomagnetic tail—the very complex situation.

We, to this day, do not have it completely understood, but we now know, we think, how to attack the problem. Single measurements don't do us an awfully lot of good anymore. Anything so interconnected, so intertwined, calls for the multiple measurements in multiple places at multiple times—that is the avenue on which we're proceeding for the future.

There are some advances occurring in our technology that work across the board to help us in space science: the computer capability; modeling, as I mentioned, for the Earth science is important for astronomy; it's important for space physics.

The computer technology is now becoming inexpensive enough that we can distribute computers, very powerful ones, to essentially every scientist laboratory.

This will also enable us to do something else. We can involve more scientists closely in space exploration. There is no need with today's modern communication and computer technology to have all our scientists traveling en masse to Houston or Goddard or any other place to conduct their research.

We envision in the future, not too many years from now, that using communications the scientist can stay in his home laboratory or her home laboratory where the students are, where they're used to working and comfortable, and participate directly through this concept we call telescience—a major advance in our thinking and one which we're structuring the Space Station Program to adapt to.

A few words on the Space Station Program: you'll hear words plus or minus about the benefits of it. I don't want to get into a discussion of why we're doing it. There are many reasons. Yet, there is no question in my mind that it will present opportunities which we will be foolhardy not to take advantage of as scientists.

There are experiments in astronomy, space physics, which require large structures, large antennas, long observing time. To use the shuttle and space station to help us maintain and repair satellites has got to be a cost-beneficial way of operating in the next decade.

A goal of astronomy is to have a set of observatories in space, not unlike some of our ground-based observatories. Looking across the spectrum, using the telescience techniques, and with the spectrum of observatories, a scientist should be able to say, "I have a problem to work on. I need to use x ray today and infrared," and, again from his home base, make those observations without ever getting out of his laboratory.

To do that requires, though, that our great investments in the space hardware, in essence, be protected. We've got to be able to keep these observatories going, to repair them, to maintain them, and that I see as a prime benefit of the space station and the continued use of the shuttle.

Toward the science program for a moment, we've been working and looking with NOAA to see if there isn't a better way to do our operational and research programs in Earth science. There's a convergence there that makes an awful lot of economic and scientific sense, which is to share the opportunities more closely than we do now.

We're looking to the polar platforms of the space station as that opportunity to bring our research and operational activities together again to get the synergism between those two programs.

The ability for the space station polar platform to fly multiple experiments for complex experiments and to service those experiments for decades is key, in my view, to the success of the Space Station Program.

Again, when you sum up what is the Space Science Program all about, it has other benefits, in my view. We're all, I believe, convinced that technological progress is essential to the economic well-being of the United States, and certainly space science has got to benefit from better economy in the country.

All science is an underpinning of technological advance. Space science is part of that. It has, in my view, a special place, not because it naturally enhances or encourages specific technology, but space science and space exploration—sometimes two different things—are part of what I believe is very attractive to our youth. It is on the forefront of excitement, of exploration, of scientific research.

The youth in our schools are excited about space science and exploration. It encourages them to take careers in engineering or science. Whether or not they ever end up as space scientists is really immaterial. It provides a basis, a reason for studying, for working—not just in the sciences, but in the humanities—to understand where is it that mankind is headed.

That, to me, is a major piece of why we're in the space science business. It, to me, is a reason why the United States must maintain a leadership. International cooperation is good. We have benefited greatly in the past with international cooperation, starting way back in the early days with Canada, the United Kingdom, with Europe, and a bit with the Soviet Union.

I worry a bit about it. I worry sometimes that international cooperation is seen by some people as strictly a mechanism by which to reduce the cost or the budget to the United States for the conduct of space science. That reduction cost is real, and that's good. But I think, as we look at international cooperation, we must continually ask ourselves: Where should we do it? What projects are really best suited for it? Where do other countries have an expertise which would even be better than ours? Where should we maintain our leadership?

I would not like to see our activity diffused totally across the board in terms of cooperation, but to take certain areas where we do maintain a strong leadership and put priority on those in our U.S. Space Science Program.

All in all, I'll conclude now and again say that space science I think has served the country well over the past 25 years, and I see no prospect for that doing anything other than serving us well in the future if we but nurture it.

Thank you.

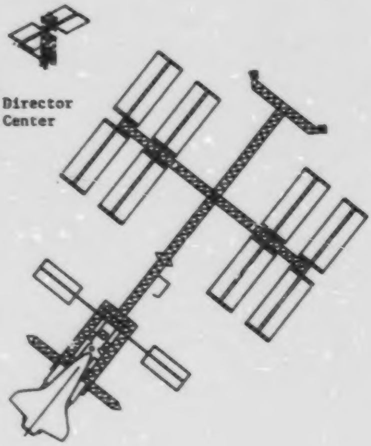
[The prepared statement of Dr. Hinners follows:]

National Aeronautics and
Space Administration

Hold for Release Until
Presented by Witness
October 8, 1985

Subcommittee on Space Science and Applications
Committee on Science and Technology

House of Representatives



Statement by: Dr. Noel W. Hinners, Director
Goddard Space Flight Center

99th Congress

Statement of

Noel W. Hinners
Director, Goddard Space Flight Center

NATIONAL AERONAUTICS AND SPACE ADMINISTRATION

before the

Subcommittee on Space Science
and Applications
Committee on Science and Technology
House of Representatives

Mr. Chairman and Members of the Subcommittee:

I welcome this opportunity to address the topic of Space Science in an atmosphere of inquiry and desire to learn. That is, after all, the basis of the scientific endeavor -- a quest for knowledge. That quest is unbounded and never ending, attributes inherently harmless, yet the world of budget constraints, competing societal and individual needs, and occasional misgivings about the practical value of scientific inquiry result in the establishment of limits and priorities. My challenge is to provide you with views of space science relevant to your decision process, a process crucial to creating the means by which space science progresses.

I worry about whether or not I am up to the task; what can I bring to you today that couldn't better be done by my colleagues actively involved in the hands-on conduct of space research? Perhaps a different perspective, for though trained as a scientist I have made a career of science management and administration, the rewards of which flow from helping create the opportunities for "practicing" scientists to be innovative and productive. That career has seen a progression of incarnations ranging from student to technical contractor to NASA to NASA Associate Administrator for Space Science to Goddard Center Director. In that time I've been exposed to a spectrum of scientific, budgeting, political, and social issues associated with space science and which span its history as measured from the launch of Sputnik in 1957.

I also had the good fortune to spend (1979-1982) a rather pleasant and informative interlude as Director of the National Air and Space Museum, an unparalleled opportunity to assess and contribute to public knowledge of and interest in space science and exploration. A side-benefit, or possibly a side-effect, was the acquisition of think-time, in which to dwell on what had transpired over the past 20 years and to sort out what in my career to do next. The end result was a re-commitment to space exploration. I just could not think of anything more rewarding, exciting or worthwhile. Let this pre-amble be fair warning that you are listening to a devotee, albeit one tempered with a conviction that it is our obligation to assure quality science and to share the excitement and fruits of our ventures with those who pay for it -- the U.S. public through you, its representatives.

I will give you my thoughts on the essential character of space science, examples of its contributions to knowledge and national well-being, its role vis-a-vis technology, and international cooperation and competitiveness. Along the way, I'll also give my assessment of future trends and needs. This could be dismissed as interesting, but just another look into a cloudy crystal ball. Possibly, but I believe that the future is largely under our control rather than subject to whims of "random events" or the wiles of nature: many things will happen because the initial steps are already in motion or because someone wants them too; let us therefore plan determinedly for our future.

Best to begin with a definition of Space Science: as stated by Homer Newell in his excellent book "Beyond the Atmosphere", space science consists of..."those scientific investigations made possible or significantly aided by rockets, satellites and space probes." Thus it is that existing scientific disciplines and more narrowly focused subdisciplines in astronomy, physics, and earth and planetary science gained opportunity to expand by dint of new technology enabling escape from the earth-bound laboratory.

For astronomy the vantage point of space opened up new regions of the spectrum quite simply by astronomers ridding themselves of the obscuring effect of earth's atmosphere. Completely new discoveries involving basic physics and hard-to-believe phenomena commenced immediately and continue to this day to come forth as we peer into new parts of the spectrum with detectors of increasing sensitivity and resolution. There appears to be no end in sight for the potential of yet further discoveries, ones helping elucidate answers to such basic questions as how did the universe and its features begin and how did it evolve, what are quasars and black holes, do black holes really exist?

For what use, you say? "Knowledge for sake of knowledge" might be a typical response but I personally find that to be a flip answer, repugnent, and providing fuel for those who accuse scientists of elitism. Rather, there is incredible excitement and elation involved in learning about the power, complexities and wonders of nature on such a cosmic scale; this is mimicked no less on the other end of the size scale by today's discoveries in the biological sciences. Obviously there is emotion involved in this; excellent! D. H. Lawrence in his 1931 "Apocalypse" rightly complained "Men are far more fools today, for stripping themselves of their emotional and imaginative reactions, feeling nothing." While Lawrence bemoans the loss of the sun as the mythological entity of old civilizations... "All we see is a scientific little luminary, dwindled to a ball of blazing gas", had he today's knowledge and scientific understanding I think he could have found a new awe and mystery far more satisfying than the make-believe of old. It is our imperative to bring the excitement and emotion of discoveries about nature to the masses who by necessity participate second-hand. My museum experience convinces me that it is possible to do so, that the public wants to understand and will support our research if we but successfully convey to them the results and their inherent excitement.

For space physics, space platforms have provided not only new places from which to peer but they allow immersion and measurement in the new medium of magnetic and electrical fields, particles, solar wind, cosmic rays and the like. The picture we now have of earth's envelope in the solar system is one for which there was only a hint pre-1957. Basic discoveries abounded in the 1960's; in the 70's and 80's the challenge has changed from one of discovering "what's there?" to a greater one of deciphering and understanding details of how the physical processes work. A mature science, some would call it, sometimes with a hint of derision. If realizing that a topic is complex, that interactions among components are strong, and that the easy discoveries are over defines maturity, so be it. But to some of us near the age of 50, maturity means wisdom, careful selection and prioritization of tasks, calling upon experience to select a fruitful experiment, and recognition of the broader implications and significance of what we do.

The results of space physics studies are, like those of astronomy largely of pure scientific interest. However, because our earth is embedded in the magnetosphere, phenomena occuring there have direct bearing on closer-in events such as aurora, disturbances in the ionosphere which disrupt radio communications, radiation damage to satellites, and quite possibly on long-term weather and climate trends. To obtain a better understanding of the environment in which we live is an added inducement for the study of space physics. So is the fact that basic physical processes occur there which cannot be duplicated in the laboratory but which are of great interest for such things as fusion energy research -- in this sense we can use the space environment as an in-situ laboratory.

The study of space physics also occurs in our planetary exploration program. The detailed knowledge built up by studying the earth's physical environment has allowed us to greatly increase our understanding of similar phenomena at Jupiter and Saturn where the data has been limited mostly to that obtained by a few transits by the Voyager and Pioneer spacecraft. In a grander extrapolation, we are beginning to see in astronomical data that some of the phenomena observed near earth may be occuring on a galactic scale.

Earth science -- geology, planetology, call it what you will. If space physics is mature, earth science by the same measures would be senile, being over 200 years old by any accounting. We know the earth to be exceedingly complex and in contrast to the space physics environment, most of the solid earth is hidden from direct view. Yet the last twenty years have seen a revolution in our understanding of the earth, how it may have formed and evolved, how its engine is fueled and moves major chunks of crust about willy-nilly. Rebirth? Rejuvenation? Yes. Made possible by several factors: the tedious buildup of immense detail over the years by many plodders (and far fewer big thinkers), detail waiting for the integrating framework of:

plate tectonics; new technology allowing us to "see" the 2/3 of the earth's surface hidden under the oceans and to peer into the interior via geophysical probing; meteorite studies; and remote and in-situ investigations of the other planets of the solar system.

The latter is a unique contribution from the space science program. Just as physical laws seem to be the same everywhere in the universe, so do fundamental geological and geophysical processes. But what we see in the earth and planets is a summation of billions of years of evolution and of a multitude of processes working at different rates on different starting materials. The ability to decipher the history of any one of the resulting complex bodies in isolation is not as good as one might wish. Our overall understanding can be enhanced however, if one has a spectrum of planets to study, each of which has evolved in a different manner. You can think of this as analagous in some sense to mathematics which requires multiple equations to solve for multiple unknowns. This approach has some times been termed comparative planetology and brings renewed focus to thinking of our earth in a planetological sense. It is not only the study of the origin and evolution of the earth that benefits from the use of space techniques; over two decades of evolution of earth-orbiting weather, atmospheric and oceanic research satellites has led to a dependence on them for making progress in eventually, obtaining a global-scale comprehension of earth systems.

Just as one learns about the general properties of terrestrial evolution from planetary studies, so too do atmospheric scientists gain greater insight into earth's atmospheric circulation and chemistry from planetary data. One does not, however, study the planets primarily to understand the earth. It is to arrive at a comprehension of the basic origin and evolution of planets per se; planetary studies are complementary to meteorite studies and to space-and ground-based astronomical investigations which are just beginning to tell us how stellar origins involve the formation of planets. For example, NASA's Infrared Astronomy Satellite has detected what is probably a disc of dust or rocky material surrounding a young star and which appears to be a progenitor of planetary formation.

The convergence of the astronomical and geological lines of evidence regarding planetary or solar system formation is a bonus of our times, brought to us solely because of our new capabilities in space. Equally astonishing is the sheer beauty of the discoveries from our planetary exploration -- the scientific beauty of the likes of the volcanism of Io, the canyons of Mars, the Red Spot on Jupiter, the rings of Saturn. Likewise, their aesthetics. Human artistry pales when compared to what nature produces -- so much so that we often seem linguistically incapable of expressing adequately the impact on our senses and emotions, reducing ourselves to primitive, feeble utterances of "wow, gee-whiz."

Where are we headed in the space sciences? Is anything new really happening or is it just more of the same thing? Not at all. In astronomy, we see that the traditional approach of labelling and behaving as "wavelength bin" astronomers (e.g., IR astronomers, X-ray astronomers) makes less and less sense. The attack on fundamental problems more and more requires observing the same event or object in a multitude of wavelengths. Our goal, then, is to create a set of long-life space observatories (physics dictates multiple detectors and telescopes) complemented by ground-based facilities, which will enable an astronomer to pick a problem, easily access any of the telescopes and make the coordinated observations. We would like those to be made from the home university or laboratory location, where the scientist interacts with the students and where the environment is most conducive to scientific productivity. The Hubble Space Telescope, to be launched in 1986, is the first of the long-life observatories, with gamma-ray, X-ray and Infrared observatories to follow. We are making progress on distributed or remote access to space observatories, the International Ultraviolet Explorer and the Upper Atmospheric Research Satellite leading the way. The remote access advances are enabled by the technology of modern communications and computers; the advent of the Space Shuttle and the Space Station make or will make possible the long-term maintenance, repair and experiment up-grade of our space observatories. The conduct of space science is rapidly evolving in style towards that traditional for other sciences.

In space physics, we clearly see that one part of the environment interacts or is closely coupled with other parts. The opportunities for major advancement in understanding thus arises in the possibility of making multiple, simultaneous space measurements in the various regions: the solar wind, the auroral zones, the electrical arcs, the radiation belts and magnetic tail. The types of measurements needed are well known, guided by the previous experiments and by a firmer theory and computational modeling than existed in the past. Theory and computational modeling are deeply rooted in work going on in understanding basic plasma (fully ionized gas) physics for purposes additional to the direct needs of space science (e.g., fusion research).

The earth sciences present a special challenge which we are beginning to comprehend: that is, interconnections, feedbacks, cause and effect dominate the behavior of the atmosphere and oceans. For very practical reasons we are trying to understand the global dynamics of earth's environment sufficiently well that we can construct predictive models of its behavior. To do so requires global-scale multiple and sumultaneous observations of the atmosphere, hydrosphere, ice caps, biota, and land masses. Space techniques using polar orbiting satellites and space station polar platforms are the sine qua nons to accomplish our objectives.

It is evident by now that technology has been and will always be key to new discoveries in space or anywhere else. Space science has not just benefitted from technological developments but has been a puller of technology. The scientist has a need and looks to the engineer to provide him with the means. The resulting new basic discoveries then become part of our store of basic knowledge from which future technologies and applications will evolve -- the two are connected and the eventual health of our U.S. technology, which inarguably is necessary to a healthy U.S. economic competitiveness, demands a healthy scientific underpinning. Where does space science fit into the overall scheme of U.S. science?

Space science is a forefront science. By that I mean it involves the making of fundamental new discoveries or significant advances in understanding non-trivial physical processes. Obviously judgements are involved here and differences of opinion abound about what is significant and what is trivial. One measure among many however, impresses me: where do the young bright students go? Science students are pretty smart -- they'll go where the intellectual action is (given reasonable financial return!)

Space science and exploration has historically attracted excellent students and researchers. It excites youth at an early age and is a stimulant to the career choices into science and engineering. It continues to do so but there are storm warnings: the U.S. historically has been a world leader. The rest of the world has progressed rapidly and collectively may pass the U.S.

Should we care? After all, science is universal, and international cooperation is traditional and has paid dividends in terms of more science results than if we were to go it alone. And science results from other free-world nations are available to us.

Mr. Chairman and Members of the Subcommittee: I do care and am worried. I favor international cooperation and healthy open national endeavors from other countries. In structuring future cooperations, however, and in prioritizing our own programs we need to look carefully at how to maintain a strong element of U.S. space science leadership. We need to assess the pace of our programs and to think more about how scientific leadership relates to technological leadership and eventual economic competitiveness. Competition is inherently healthy and stimulating and, because we cannot do everything, we must assess our national strengths and weaknesses and put our resources behind the winners. I believe that U.S. space science has served the country well both intellectually and in bringing about practical applications. Prospects are excellent for continuing to do so. It is gratifying to work with you towards that goal.

Mr. Nelson. The observatories that you refer to, you're talking about the four great observatories that we're looking forward to having—the Hubble, the x ray, the gamma ray, and the infrared?

Dr. Hinners. Those are the four commonly talked about; that's correct.

Mr. Nelson. All right. And tell me some more about telescience. You talk about the sharing of data and ideas back and forth from one laboratory to another.

Dr. Hinners. Telescience is a much broader concept than that. It's one, for example, in which a principal investigator would remain in his laboratory and communicate through either a communications satellite or land lines to NASA control center and then up to the satellite and run his experiment from home.

We are constructing the upper atmospheric research satellite to operate that way. Our 11 principal investigators will be in their home laboratories for the conduct of that mission. It will save immense travel. They'll be there with their students. They can get the students involved, a very important part of the continuing effort in space science—to keep the young people coming in to feed the system with the new scientists and engineers in the future.

Mr. Nelson. Mrs. Meyers.

Mrs. Meyers. No questions.

Mr. Nelson. Mr. Smith.

Mr. Smith. No questions, Mr. Chairman.

Mr. Nelson. All right. Let me—what do you think about Dr. Sagan's testimony about Soviet-American cooperation?

Dr. Hinners. I'm very much in agreement with Dr. Sagan's views on that. I am a subscriber to the belief that only good can come out of contact and communications. I think the scientific area is one in which we can make major headway; that the science per se is apolitical; and I also dream on occasion that, in terms of the manned space flight—let me use the term until we come up with a better one collectively—I would much rather see the United States and the Soviet Union diverting money from defense activities into exploration of the Moon or Mars or asteroids.

I get worried about defense in all countries. If I might take the liberty here of expanding on that a minute:

Our schools do a wonderful job sometimes of challenging the imagination, of making people curious. Our students when they come out are looking for challenges. The engineers want to do exciting technology. Where are the opportunities for exciting technology? The greatest opportunity for exciting technology right now is in large part in the defense industry.

We've got somewhat of a built-in dilemma. And I'd like to see us make our space program exciting enough to compete with that, and on a worldwide basis draw some of our best talent into human explorations.

Mr. Nelson. Indeed, I would say that, of all agencies being attractive for young engineers, that NASA certainly ought to be one of them, aside from the question of the pay scales but as far as challenge, particularly looking to the future.

Do you have a few minutes that you can stay with us?

Dr. Hinners. Yes.

Mr. **Nelson**. All right. Can I get you just to shift down to the end of the table, and may I ask the panel to come up.

Noel, if you could just pull up an extra chair there.

Dr. **Hinners**. Yes.

I have to leave about 3 p.m., Mr. Chairman.

Mr. **Nelson**. OK, fine.

If the panel—Dr. Van Allen, Dr. Lanzerotti, and Dr. Krimigis——

Dr. Van Allen, what do you think about Dr. Sagan's comments?

Dr. **Van Allen**. Well, sir, I consider Carl one of my most interesting friends, so I'm always entertained by anything he has to say. But if I try to lower myself back to Earth, I come to the following conclusion: that some people I know, including yourself and Senator Matsunaga, have the feeling—and I'm very respectful of that feeling—that if we can execute a genuine collaboration, operational collaboration, with the Soviet Union in space, that this will ease the political tension between our two countries. And I'd like to join in that feeling, but I must say I'm skeptical of that. I don't think we have enough leverage to pry out the basic political conflict between our countries.

My own way of looking at it is that only if the political tension is eased in some more fundamental, powerful method will we ever achieve an operative collaboration in space.

I might say I attended the July conference of the Planetary Society and the AIAA, and I thought it was great good fun, but I couldn't help but lower myself back to Earth after the end of the day.

Mr. **Nelson**. Well, that's part of the reality that we face, but my attitude is that we have to try. And space is an environment in which adversaries on Earth can come together and cooperate. And so I'll be interested to share some of our conclusions when we get back from Russia with you.

Well, why don't we approach it this way: we have until 3 o'clock when Dr. Hinners has to depart. So why don't we take some opening statements from you all in a more informal, conversational way, and then we might get some dialogue going here before Dr. Hinners needs to—and then if we need to go further with some of our statements individually, we will.

So let's start with you, Dr. Van Allen.

STATEMENT OF DR. JAMES A. VAN ALLEN, DEPARTMENT OF PHYSICS AND ASTRONOMY, UNIVERSITY OF IOWA, IOWA CITY, IA

Dr. **Van Allen**. All right, sir. Thank you very much, Mr. Chairman.

I take it—I have submitted for your committee a written statement which——

Mr. **Nelson**. And it will be part of the record.

Dr. **Van Allen** [continuing]. Which does, however, take longer than you indicated you wish me to take at this point. And I would be better organized if I read it, but let me be somewhat less organized and not read it by giving some of what I think are its main points.

I'm, along with a number of other persons who are here present today, I'm participating in the Major Directions for Space Science, during the period 1995 to 2015, which is chaired by Professor Donahue, who also was or is—is or was present here this afternoon.

And the thing that I think is most impressive from our deliberations, which have been going on now for about 18 months and will continue for another 8 months or so—we hope to make a final report for general consumption of whoever may be interested by the end of June of next year, June of 1986.

The impressive thing about all this is the magnitude of our agenda, our aspirations. And we haven't been charged with really costing these out, but I think anyone who did so would find that they exceeded the gross national product on an annual basis, or at least a substantial fraction of it. I think that's the message, one message, I would like to mention.

Now the Space Act, of course, calls for—well, the expansion of human knowledge of phenomena in the atmosphere and space is the first of three principal objectives that the Congress declared to be the purpose of this act and the purpose of NASA, which it established. There are five other objectives listed in the act, all of which I would assess as being supportive or secondary in nature. The three objectives lead the list.

Now the Space Act says nothing whatever about the relative emphasis on these three major objectives, nor the total magnitude of the effort by the United States in the whole enterprise, and these matters, of course, have been worked out at a political-societal level, which is probably the way things should be, rather than being spelled out in the basic bit of legislation.

Now the thing that I'm most impressed about as a realist these days is the following: that following the great Apollo peak of expenditures for civil space activities, the space budget of NASA, annual budget, fell by almost a factor of 3. And for the last 10 years it's been almost the same every year for the last 10 years, roughly in constant, in normalized dollars.

So, as an empirical observation, which since I'm an experimentalist I believe in what I can observe, that's a plain fact at the present time.

Now the other principal element of our outlook in a general sense is what I call the glowering Federal deficit, and I don't know if and when Congress intends to do anything about this, but it seems to me it must be done sometime soon.

Now if I take those two facts together, I can't help but conclude, despite the efforts of the National Commission on Space and many others of us, that we have to confront what is generally called roughly a zero-sum game, meaning that anything we increase, any element of NASA's budget which is increased, results in a corresponding equal decrease elsewhere. So that's the reality as I see it.

So I'm bothered very much. I made a statement this morning. Let me just pick a piece of that out here, if I may. Anyone who was at the caucus this morning, please pardon me. I have essentially the same statement this afternoon. The reason is I haven't changed my mind since lunch.

The—in the State of the Union Message—in his State of the Union Message on January 25, 1984, President Reagan gave a ringing endorsement of the space activities and announced his approval of the development of a permanently manned space station as a major national objective during the next decade. It was pleasant to hear of his interest in space, but I found his rationale for this $20 billion undertaking so speculative and so poorly founded that no one of lesser stature would have dared mention it to an informed audience. And I can assure you that if NASA were to receive a research proposal of this quality from a university scientist, it would be rejected promptly, even if it were for only $20,000.

Now the President's accompanying assurance to NASA of only a 1-percent-per-year real budgetary growth implies, again, hard times for space science and applications for many more years if the planned expenditures for the space station proceed as presently projected.

So that's my basic concern these days, and Congressman Green, who was present this morning, I think joined in this feeling. And he thought it was even worse than I pictured it. I think that's essentially the message he gave to us this morning.

So that's the conflict in my mind: on the one hand, we have these very well-developed and I think defensible aspirations in space science and applications; on the other hand, we're faced with a more or less zero-sum game. And I labor under the difficulty of harmonizing these two elements in my mind.

[The prepared statement of Dr. Van Allen follows:]

Statement for the

House Subcommittee on Space Science and Applications, 8 October 1985

by

James A. Van Allen
Department of Physics and Astronomy
University of Iowa

Mr. Chairman:

I am grateful for this opportunity to make some remarks on the future of space science in the United States.

I am participating in the National Academy of Sciences' current study on Major Directions for Space Science during the period 1995-2015, chaired by Professor Thomas Donahue. We are developing a rich agenda of aspirations in the fields of my special interest, namely solar-terrestrial and planetary physics. We understand that one of the central elements of our near-term agenda, the International Solar Terrestrial Physics program, has been approved recently by the Administrator of the National Aeronautics and Space Administration for a full fledged start in FY 1987. We hope that the President and the Congress will also support this program, which is basic to further advances in space plasma physics and the understanding of the radiation belts and magnetosphere of the earth, a very complex and fascinating physical system that we discovered in 1958 with the first successful U. S. Satellite, Explorer I.

Inasmuch as several of my colleagues are prepared to describe specifics of our program, I decided to make some remarks of a more general nature.

The proper perspective on science in general and space science in particular is represented by the title of Vannevar Bush's famous 1945 Report to the President, "Science, The Endless Frontier".

Most scientists are delighted whenever their work leads to useful applications. The history of science is replete with examples. The invention of the transistor some 38 years ago is a prominent one. This enormously useful device has revolutionized telecommunications, high speed computing, and kindred fields; yet it emerged from the arcane discipline of solid state physics, which most persons have never heard of, even today. This is an illustration of the fact that fundamental science, however little known to the general public, provides the basis for a vast variety of practical applications.

But, in some sense, useful applications are only a by-product of science. Science itself is basically an intellectual endeavor which has a life of its own, quite apart from potential applications. It derives its primary motivation from curiosity and the spirit of systematic investigation. So it is with space science and with astronomy.

Science, as defined by its practitioners, is largely a non-political subject because very few persons are opposed to it. But the federal funding of science is another matter -- especially since its benefits are difficult to quantify. The justification for the public support of science lies in finding a level of effort that enjoys a consensus of worthwhileness among thoughtful and influential citizens. Hence the level of effort is a political subject.

In the famous National Aeronautics and Space Act of 1958, the Congress declared "the expansion of human knowledge of phenomena in the atmosphere and space" to be the first of three primary objectives of NASA, the agency that this act created. The other five named objectives are of a secondary or supportive nature.

I recall with pleasure testifying before the joint House-Senate Select Committee while it was writing this legislation. The co-chairmen were then Senator Lyndon B. Johnson and Congressman John W. McCormack, later Speaker of the House.

One of the great weaknesses, or perhaps strengths, of the Space Act is that it assigns no relative emphasis to the three primary objectives nor does it specify an appropriate level of overall support. These vital matters have been worked out over the years by our complex political-societal processes. The remainder of my remarks is devoted to these matters.

Following the great Apollo peak in national space expenditures in the mid-1960's, the annual budget for civil space activities declined by a factor of three (in normalized dollars) and has been essentially constant for the past ten years. Because of the latter empirical fact and further because of the glowering deficit in recent federal budgets, it is difficult if not impossible to responsibly foresee a significant upward trend in space expenditures. I come to this conclusion regretfully and in full knowledge of the efforts of newly appointed members of the National Commission on Space and many others of us to devise space objectives that will engage enhanced public support. Hence, at best, we seem to confront a zero-sum game. By this term I mean that any increase in one element of the NASA budget inevitably results in an equal decrease elsewhere.

In the early years of NASA's history, space science and applications represented over 20% of its budget. This percentage has now been nearly restored after a prolonged slump to a low point of about 9% during the Apollo epoch. I applaud this recovery and acknowledge that it has been accomplished despite massive expenditures on development of the space shuttle since 1971.

Nonetheless, I believe that we could have done much better, absent the shuttle program. By the early 1970's the United States already had a stable of reliable, high-performance, expendable launch vehicles -- the Atlas, the Scout, the Delta, and the Titan. These vehicles and well developed upper stages were adequate and are adequate to perform almost all of the significant functions of the shuttle, and can do so more reliably and at much less cost. With these launch vehicles available, the decades of the 1970's and 1980's could have been and could now be periods of markedly greater emphasis on space science and applications, which after all are the principal end products of NASA as an agency.

I may also note that there has been a marked, qualitative change in the nature of NASA's scientific endeavors during its 27-year history. In recent years, emphasis has been on major missions -- those in the billion-dollar category -- best illustrated by the Viking, Space Telescope, Voyager, and Galileo projects. Such missions represent a trend of space science toward ever greater complexity and sophistication and do, indeed, have high merit. But they tend to squeeze out more flexible and much less expensive undertakings that have historically been highly productive and have, moreover, nurtured space science on a broad national basis in a way that is less well done by major projects concentrated at a few research establishments. I believe that this matter deserves sympathetic congressional consideration.

In his State of the Union address on 25 January 1984, President Reagan gave a ringing endorsement of space activities and announced his approval of the development of a permanently manned space station as a major national objective during the next decade. It was pleasant to hear of his interest in space but I found his rationale for this $20 billion undertaking so speculative and so poorly founded that no one of lesser stature would have dared mention it to an informed audience. I can assure you that, if NASA were to receive a research proposal of this quality from a university scientist, it would be rejected promptly -- even if it were for only $20 thousand.

The President's accompanying assurance to NASA of only a 1% per year real budgetary growth implies, again, hard times for space science and applications for many more years as planned expenditures for the space station increase.

I have no doubt that significant uses for a space station can be found, if such a facility were to suddenly appear by zero-cost magic. But in the real world, I should much prefer to see an incremental approach to such uses, pacing the approach to those clearly demonstrated purposes that cannot be served better in other ways. Under such an approach, space science and applications could be expanded gradually toward the desirable goal of some 50% of the NASA budget. Only in this way can the United States retain its now rather fragile international leadership in durable achievements in space.

Mr. Nelson. Dr. Hinners, you have already, in effect, given the opposite argument to that. Do you want to share any further comments with regard to what Dr. Van Allen——

Dr. Hinners. No. I think the difference between Dr. Van Allen and me may be on my presumption that forces greater than I control will see that certain things happen. So I believe that there will be a space station, and actually I do support it for reasons which Dr. Van Allen probably does not.

Regardless, I see it as incumbent on the science community to do everything it can to influence early the design of the space station, so that, when it comes, it is something which we can use to our advantage, rather than being at the mercy of whatever the engineering community decides it is they'd like to build.

Mr. Nelson. And is that influence proceeding, in your opinion?

Dr. Hinners. Yes; it is. I believe we are having an impact on the design. It's something we need to watch very carefully.

The budget crunches are going to be there; there's no doubt. I worry that the first thing to disappear when that comes will be the user accommodations. It is a very critical time coming, and to watch to see that it evolves into a space station that is something that the user will want to use come the nineties.

Mr. Nelson. Dr.—yes, sir?

Dr. Van Allen. Mr. Chairman, I'd like to say, sir, that I have absolutely no objection to the space station. I enjoy the concept. If I were an aeronautical engineer right now, there's nothing I would like to work on more than the design and development of the space station, which is one of the most challenging technical undertakings NASA has.

My only reservation is the one I mentioned, the zero-sum game, and the fact that I think we cannot walk around the streets in this rosy fog wearing rose-colored glasses and believing it's anything different.

Mr. Nelson. You know, we do play an interesting game up here. We always try to make it add up, and it doesn't add up. I mean, who would have thought that we would have suddenly in the last 4 years jumped from a $1 trillion national debt to a $2 trillion deficit?

Dr. Van Allen. That has increased.

Mr. Nelson. What do you think, Dr. Krimigis, about the influence of sciences upon the design of the space station?

Dr. Krimigis. Well, as I'm sure, Mr. Chairman and committee members, you're aware, the Space Science Board did speak to that issue, and it was certainly true that the scientists were not a driving force in the development of the space station concept.

I, like Noel Hinners, believe that some things are inevitable in this world and that we, as scientists, do not necessarily have a great deal of influence on such occurrences, which are basically political. And, therefore, once certain realities dawn on us, we have to accommodate our thinking and actions accordingly, and our job then is to make sure that we maximize the science that can be obtained from the system.

Nevertheless, the experience with the shuttle has not been good. We were not as successful in maximizing the science out of the system as original promises indicated, and we need to be ever vigilant that the same experience is not repeated in the present situation. And we rely on the sympathetic ear of this committee and its counterpart in the Senate to make sure that science is not overlooked in the process.

Mr. **Nelson**. How about you, Dr. Lanzerotti?

Dr. **Lanzerotti**. I sympathize with the statements of Tom Krimigis on this matter.

The space station can provide extraordinary opportunities for certain of the space sciences and—provided the resources for the science proceed in parallel and provided that the scientists have a significant input, as I believe they are doing at the present time through the—NASA has a subcommittee of the NASA Advisory Committee on Space Science and Applications who are looking at that very importantly.

I think it's important to recognize, however, that the space station, as it presently is conceived and as it will exist for the first few years of its life, operational life, is in one location in space at an altitude that corresponds to the distance from here to Wilmington perhaps, except it happens to be up instead of horizontal.

At that altitude one can do certain space science. One can implement these astronomical and astrophysical observatories, a tremendous advance in our understanding of the universe.

One will be able to do planetary science from that location with those telescopes, but one must recognize that one cannot do planetary science in situ. One cannot land on a moon of Jupiter or send a spacecraft around Pluto at the space station orbit. One may ultimately be able to utilize the space station for constructing interplanetary explorers to study the Sun and the interplanetary medium from the space station, but it may be a decade or two or three. And we have to ask ourselves and be—make sure we understand the implications of perhaps a long hiatus in a planetary exploration program.

Also, we cannot understand the energy conversion process in the Earth's magnetotail, a process very much like exists in stars throughout the universe and perhaps in the centers of some galaxies. Perhaps the same kind of plasma physics, although on a smaller scale, are operative in the Earth's magnetosphere and in the Sun. And we cannot explore those at an altitude that corresponds to the distance between here and Wilmington.

Now—so before we devote all of our resources to that, we have to make sure where our science priorities lie, and therein I sympathize to some extent with Dr. Van Allen's concern—that if all the resources go into the space station, and even some of the science that may be associated with that, the United States will lose its leadership role in many of the other science disciplines, including the exploration of the moons of Mars, which the Russians are setting out to do, or lose our leadership in the exploration of the outer planets or our leadership in the near-Earth space.

Mr. **Nelson**. What message would any of the three of you give to this committee that we ought to share with our Soviet hosts, any items on the agenda that you think ought to be placed on the table?

Dr. **Lanzerotti**. I'm not sure I know about your plan. Would you explain that, please?

Mr. **Nelson**. We're taking off Thursday night and we are going—our host is the Soviet Academy of Sciences. I've already met with Dr. Sagdeyev when he was here in the United States. We will be, in addition to having discussions with the academy members, we will be visiting the cosmonaut training facility at Star City and the mission control area at Kaliningrad.

Overall, we will be conducting this venture in the spirit of an anniversary celebration of Apollo-Soyuz. And we'll be taking two of our three American astronauts with us to join with the two Soviet cosmonauts there for such an anniversary celebration.

The purpose of the trip is to discuss cooperation in space with the Soviets. And from my standpoint, as the leader of this delegation, we'll consider anything put on the table as a door opener to discussions, ultimately perhaps to be picked up, if not already underway, by various sources within the administration.

Dr. **Krimigis**. Mr. Chairman, one thought that occurs to me is that to begin the process we need to begin with well-defined programs that perhaps ourselves and the Soviets may have in the planning stage at the present time. And one of the programs which, according to Aviation Week anyway, is supposed to be submitted to the Congress is the International Solar-Terrestrial Physics Program, which at the present time consists of participants from the United States, the European Space Agency, and Japan. And each country, Japan and Europe has a substantial contribution.

Now the Soviets themselves, it's our understanding from previous statements by Dr. Sagdeyev, have a program called Interball, which has certain complementary aspects to it. And, in fact, I was informed that Dr. Sagdeyev, when he visited the United States last month, when apparently you got together with him, specifically made a proposal to place a Soviet spacecraft on the night side of the Earth at the distance which would be complementary to our program as is presently planned.

So one possible way to proceed is to bring that to their attention, because they're apparently aware of it, and this approach also sidesteps some of the rather thorny issues that have surfaced about things such as technology transfer, for example, because we do our spacecraft; they do theirs. They place at a given place; we have ours where they are. And then in the end, what we share is the data, which is the ultimate objective of doing this work. And using the complementary data, we obtain better understanding of the Earth's environment. So that would be my suggestion as a possible topic.

Dr. **Hinners**. To the same topic—I, too, read Aviation Week. I recall the cometary mission that recently occurred, which Bob Farquhar at Goddard designed. Bob has also seen that there might be an opportunity with the Soviets on their polar obiter, lunar polar orbiter.

Ten years ago, after the Apollo missions, Vineogradov and I talked about that. The desire at the time would be to have two satellites at the Moon—one the geochemical orbiter and the other relay satellite which could track it on the lunar far side, could do the gravitational tracking. You've got to have two satellites to do that.

Dr. Farquhar has come up with a scheme as he was going around the beltway a couple of weeks ago of taking a——

Mr. **Nelson**. Is that where he does his thinking? [Laughter.]

Dr. **Hinners**. Yes.

Mr. **Nelson**. It must be mighty productive considering what he's already done.

Dr. **Hinners**. Well, he thinks just for a couple of hundred pounds of propellant on the ISTP spacecraft that's out in interplanetary space, we can for half a year put it in lunar orbit to track the Soviet spacecraft, and then when that mission is done bring it back out and put it back where it's supposed to be for the plasma physicists.

Mr. **Nelson**. Dr. Lanzerotti, any comment on this? We'll get back to the regular format where you all can talk, but——

Dr. **Lanzerotti**. Well, I think that there are certainly differences in collaborating with the Soviet Union than in collaborating with our western allies in Europe, the European Space Agency, and our Japanese colleagues.

I think that the United States has not always been a very good partner in space collaborations with our Western allies. We have delayed programs. We have canceled programs, such as the International Solar Polar Mission, now called Ulysses. And I think it would behoove us to learn from past mistakes and, if we did proceed with new ventures with the Soviet Union, be they something as grandiose as a manned Mars mission ultimately or, probably in the near-term, the ISTP Program, which I think is ideal for such a thing, we should make sure that our political system allows us to proceed in an orderly fashion; that our international collaborators don't suffer great harm to their programs and to their financial investments, be it the Soviet Union or western Europe.

Mr. **Nelson**. Dr. Hinners, I know you have to leave. Thank you for being with us very much.

Dr. **Hinners**. It's a pleasure. Thank you.

Mr. **Nelson**. Now let's give Dr. Krimigis and Dr. Lanzerotti a chance to make their statements. And, if you would, your written testimony will be a part of the record and just share with us from there, and then we'll get discussion and questions from the members.

STATEMENT OF DR. STAMATIOS KRIMIGIS, APPLIED PHYSICS LABORATORY, JOHNS HOPKINS UNIVERSITY, LAUREL, MD

Dr. **Krimigis**. Well, thank you, Mr. Chairman.

I gather from the previous comments that you are interested in a rather brief statement.

I see my main job as a member of the panel to talk to you about the recent report on the priorities on solar system space physics with the Academy's Space Science Board, as approved and published, and I believe you have a copy of that in your possession.

The rationale for that actually has been in existence for quite some time. And, in fact, this committee, your committee, included a statement in its report that was issued on April 15, 1983, authorizing appropriations to the National Aeronautics and Space Administration Act for fiscal year 1984, which stated that it recognizes that the Earth and its space environment contain coupled phenomena that need to be studied as a system from the Sun and its plasma environment to the Earth's magnetosphere, atmosphere, and biota.

At about the same time the Space Science Board of the Academy recognized the need for a broad-based strategy on this vital area of research and instructed its Committee on Solar and Space Physics to establish the highest priority scientific objectives and recommend a plan of appropriate missions which address these objectives for implementation over the next decade.

Now the CSSP was instructed to use the methodology that was developed in two reports that are well known to this committee; namely, the "Priorities in Astronomy and Astrophysics for the 1980's," the so-called Field report, and I think Dr. Field is here today, and the "Planetary Exploration Through the Year 2000" that was published by NASA in 1983.

At about the same time Dr. Keyworth, the Science Advisor to the President, in his address to the American Astronomical Society on January 9, 1984, praised the astronomy and astrophysics and planetary explorations report and then stated that he expected that the scientific community had to really face up to the issue of costs and priorities and decide what programs they wanted to implement first. And it was in that spirit that the committee then undertook this study which I am about to describe to you.

Now you may decide, after you hear what the committee had to say, that we are walking around wearing those rose-colored glasses that Dr. Van Allen talked about. I hope that it is not quite as bad.

But before I go on to the recommendations of the report, let me say a few words about the subject. As we all know, space physics as a discipline begun with the launch of the first Earth satellites and the discovery by Dr. Van Allen of the radiation belts.

The phenomena associated with this field of study are among the earliest recorded observations in many parts of the world. The ancient Greeks were puzzled by so-called fire in the upper atmosphere, which we now call aurora. I wish I could claim that I heard this from my great-great-great-grandfather, but that is not so.

There are, in fact, several possible references to the aurora in ancient Chinese writings before the year 2000 B.C. And at the risk of incurring the wrath of some folks here, there are, in fact, passages in the first chapter of Ezekiel in the Bible with vivid descriptions of what some scholars now recognize as probably auroral formations.

The observations of sunspots by Galileo in 1610 led to the 18th century discovery of the 11-year solar sunspot cycle and the recognition that there was a connection between sunspot variability and auroral activity.

The large reduction of sunspots during the second half of the 17th century, during a period of unusually cool weather in Europe, suggests the tantalizing connection between some aspects of solar activity and climate.

The diversity of environments represented within the discipline of solar and space physics demands a rather broad range of programs. We need major missions to address global aspects of the solar-terrestrial environment. We need very specific missions, such as the Explorer spacecraft, to address very specific problems. We even need balloons and rockets for experiments of opportunity.

The missions identified by the Academy committee have been selected on the basis of the priority of the science questions which can be addressed, the technological readiness, the programmatic feasibility, and, above all—and let me stress that because it's in many respects similar to what the astrophysics priorities report and the planetary priorities report used as a guide—the necessity to fit within an overall budgetary level for the entire discipline.

Using these guidelines, the committee has developed a list of recommendations, and I will only read four. They are all in the statement.

First and foremost, the continuing implementation of the upper atmosphere research satellite, the so-called UARS Program, and the solar optical telescope, which are currently approved and money being spent on both of these programs by the agency.

Second was to begin development of the International Solar-Terrestrial Physics Program, which I spoke of earlier as the highest priority new start. And, needless to say, we are pleased to see the rumors in Aviation Week about that—hopefully, the Congress will act favorably on that.

Third, to plan for later free-flying missions to carry out the technological development they require. These are the solar probe, to be launched in 1995 or thereabouts, and the solar-polar orbiter.

And, finally, from the list that I will read, to develop facility-class instrumentation for first use on the shuttle and later evolution into space station-space platform instruments, and to develop mission operations and data analysis plans for these observatories.

The committee feels that the program outlined here will result in a healthy and vigorous research effort and would allow the United States to maintain and, in fact, reclaim in some areas its leadership at a time of increased international attention to this area of science.

Our national interest will be well served by increasing our efforts to understand our space environment. The program envisioned in this report is scientifically challenging, technologically achievable, and reflects the natural evolution of a scientific field that was the first to be explored when the Nation ventured into space.

I will not comment on these programs, except for one: the solar optical telescope, which is deemed by the scientific community to be of essential importance to future progress in solar physics.

It is fair to say that delays in implementation of this program will retard progress in this vital field of research for many years to come. I do, therefore, urge that the Congress appropriate the necessary funds for orderly development of this most vital instrument.

Now the overall priorities and the program described by the CSSP is evident in the chart which I have with the testimony which you can look at, on the last page. The overall program, it is at about $300 million per year in fiscal year 1986—can be accommodated under a more or less constant funding level in the range of $300 to $400 million per year through the end of the century.

The program includes not only intensive, well-defined missions, but also exploratory missions, such as the solar probe spacecraft, which is intended to approach and explore the vicinity of the sun within a distance of approximately 2 million kilometers above the Sun's surface.

There is one other issue that I would like to address which has to do with the organization of this program. As is evident from the report and the chart, the program in solar system space physics is broad based and multifaceted, and, as such, the committee recognized that an increased level of coordination is necessary to be able to implement it in an effective and coherent manner.

Now perusal of the organization chart within NASA reveals that there are at least four separate divisions within the Office of Space Science and Applications that have various pieces of the program. And the recommendation of the committee—and I should say it is pretty much unanimous throughout the scientific community—is that an increased level of coordination is necessary by probably establishing a division on solar terrestrial physics, which in fact existed in the agency prior to the reorganization of 1980.

So it is my hope, Mr. Chairman, that the Congress, through this subcommittee, will offer some guidance to NASA on this particular issue.

Now let me summarize by stating the basic goals of solar system space research which are as follows:

To understand the physics of the Sun and the solar system and the magnetospheres, ionospheres, and upper atmospheres of the Earth, the other planets, and the comets. So this is our first goal which is basic and fundamental.

The second is to study the interactive processes that generate solar variations and link these to the Earth, because such processes have useful applications in many areas of human endeavor.

It is essential to recognize the enduring nature of these research goals that, in turn, dictate a commitment to long-term funding as the report has recommended.

First, there are the basic physics/astrophysics connections. Because of its proximity, the Sun is the only star whose interior structure and atmospheric physics can be studied at high resolution, thereby providing unique information about physical processes important to all stars.

Second, there are important connections between solar and space physics and the Earth system studies. For example, variation in solar luminosity and/or irradiance may well affect the Earth's weather and climate; stratosphere and mesosphere ozone responses to incoming charged particles have been observed and documented; variations in solar output of radiation and particles have substantial effects on the magnetosphere, ionosphere, and upper atmosphere. These issues Dr. Lanzerotti is likely to address in his testimony.

Let me conclude, however, by saying that not only is solar system space research a basic research enterprise, but it has important practical aspects with many examples which you will hear later from Dr. Lanzerotti. These include space vehicles which must function continuously in the near-Earth environment subject to the dynamic variations of the Sun, of the magnetosphere, and the upper atmosphere.

In addition to the effects of the dynamics on the electronic and power systems of spacecraft, these aspects of the near-Earth environment become particularly important in view of the planned long-term presence of man in space.

The complement of programs outlined in this report will allow us to model the global geospace environment and will thus allow us to develop a global predictive capability. This, in turn, should permit substantial improvements in our abilities to operate all space-based systems in the near-Earth region.

Thank you very much, Mr. Chairman and members, for your attention.

[The prepared statement of Dr. Krimigis follows:]

TESTIMONY ON SOLAR SYSTEM SPACE PHYSICS

Submitted to

Subcommittee on Space Science and Applications
Committee on Science and Technology
United States House of Representatives
Ninety-Ninth Congress

by

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Chairman, Committee on Solar and Space Physics
Space Science Board
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October 8, 1985

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BIOGRAPHY

Dr. S. M. (Tom) Krimigis was educated at the Universities of Minnesota (B. Phys., 1961) and Iowa (M.S., 1963; Ph.D, 1965, both in Physics). He joined the Applied Physics Laboratory/The Johns Hopkins University in 1968, where he serves currently as Chief Scientist in the Space Department. He has been Principal Investigator on several NASA spacecraft, most recently on the AMPTE Satellites (which produced the first artificial comet on December 27, 1984) and on Voyagers-1 and 2 to Jupiter, Saturn, and Uranus. He has served on several National Academy and NASA Committees, is a member of the Academy's Space Science Board and chairs the Board's Committee on Solar and Space Physics.

Congressman Nelson, and Members of the Subcommittee, I submit this statement to the Subcommittee in my capacity as Chairman of the Committee on Solar and Space Physics of the Space Science Board, National Research Council (NRC). I will describe an implementation plan for priorities in Solar System Space Physics, a study which the NRC has approved for publication recently.

CONTENT OF SOLAR SYSTEM SPACE PHYSICS

Man's wonder at the manifold phenomena in the environment and the drive to understand and use them have led to modern science. To understand the relationship between natural events on the Earth and changes in the Sun has been one of man's lasting intellectual quests. The scientific discipline we now know as Solar Systems Space Physics is the modern culmination of efforts to comprehend the relationships among a broad range of naturally occuring physical effects including solar phenomena, terrestrial magnetism, and the aurora. Understanding the solutions to these basic physics problems requires the study of ionized gasses (plasmas), magnetohydrodynamics, and particle physics.

Space physics as an identifiable discipline began with the launch of the first Earth satellite in the late 1950's and the discovery in 1958 of the Van Allen radiation belts. The phenomena associated with this field of study are among the earliest recorded observations in many parts of the world. The ancient Greeks were puzzled by "fire" in the upper atmosphere that we now call the aurora; there are several possible references to the aurora in ancient Chinese writings before 2000 BC; there are also passages in the first chapter of Ezekiel in the Bible, with vivid descriptions of what some scholars now recognize as probable auroral formations.

The observation of sunspots by Galileo in 1610 led to the 18th century discovery of the eleven-year solar sunspot cycle and the recognition that there was a connection between sunspot variability and auroral activity. The large reduction of sunspots during the second half of the 17th century during a period of unusually cool weather in Europe suggests a tantalizing connection between some aspects of solar activity and climate.

This possible link between solar activity and terrestrial phenomena could not be studied in detail until this century. We now know that in addition to the atmosphere that surrounds us, there exists a region, at higher altitudes, consisting of an electrically conducting plasma permeated by the Earth's magnetic field. It is called a "magnetosphere" because its structure and many of its processes are controlled by the magnetic field. Since the early years of the space program we have learned that the Sun has its own magnetosphere consisting of a hot (million degrees) magnetized plasma wind called the solar wind that extends beyond the orbits of the planets and fills interplanetary space, forming a distinct cavity in the nearby interstellar medium, the "heliosphere".

Using knowledge gained over the past 25 years, we can now begin to identify some of the physical mechanisms linking the Sun to our near-Earth environment. For example, motions in the convective layers of the Sun are believed to generate the solar magnetic fields and solar wind variations; these in turn affect the Earth's magnetosphere and regulate the amount of plasma energy incident on the Earth's polar caps. Associated magnetospheric activity drives strong winds in the upper atmosphere and may influence the dynamical and chemical composition of the mesosphere and the stratosphere as well. The upper atmosphere in turn is the major source of heavy ions in the magnetosphere. Further, current research suggests that small percentage changes (about 0.5%) in the total energy output of the Sun may influence short-term terrestrial climate. These and other speculative suggestions should be addressed as part of the comprehensive research program in solar-terrestrial physics because of their potential importance for the Earth. Indeed, the Earth and its space environment contain coupled phenomena and need to be studied as a system from the Sun and its plasma environment to the Earth's magnetosphere, atmosphere, oceans, and biota. This chain of interaction was recognized by this Committee in its report issued on April 15, 1983, authorizing appropriations to the National Aeronautics and Space Administration for fiscal year 1984 (Report #98-65, page 20).

RATIONALE FOR A PRIORITIES STUDY

The Space Science Board of the NRC having recognized the need for a broad-based strategy for this vital area of research, instructed its Committee on Solar and Space Physics (CSSP) to establish the highest priority scientific objectives and to recommend a plan for appropriate missions which address these objectives for implementation over the next decade or so. The CSSP report was expected to follow the methodology of two reports that had received wide acclaim for establishing priorities in related fields of research, namely the report on "Astronomy and Astrophysics for the 1980's", published by the National Academy of Sciences in 1982, and "Planetary Exploration Through Year 2000", published by NASA in 1983. Soon thereafter, Dr. George Keyworth, Science Advisor to the President, in his address to the American Astronomical Society on January 9, 1984, expressed his appreciation for the aforementioned reports and called specifically for a similar priorities study in the area of solar-terrestrial research. The Committee on Solar and Space Physics worked diligently on this task for over a year and produced a report entitled "An Implementation Plan for Priorities in Solar System Space Physics" which was approved by the Space Science Board in November 1984 and by the National Research Council in the Spring of 1985 for publication during this past summer.

PRINCIPAL RECOMMENDATIONS OF THE REPORT

The diversity of environments represented within the discipline of Solar and Space Physics demands a broad range of programs to implement the stated science objectives. Major missions are necessary for detailed studies on a global scale, as well as for exploration of previously unexamined regions (for example, the environment near the Sun). Moderate missions (such as Explorer-class) are needed to attack specific, detailed problems. Facility-class instruments developed for higher resolution measurements on Shuttle should evolve toward Space Platforms that would become part of the Space Station concept. Finally, balloons, rockets, and experiments of opportunity on Shuttle are necessary for quick response techniques. The missions identified below have been selected on the basis of the priority of the science questions which can be addressed, technological readiness, programmatic feasibility, and above all, the necessity to fit within an overall budgetary level for the entire discipline. Using these guidelines, the CSSP has developed the following recommendations:

A. Continue implementation of the following programs:
Upper Atmosphere Research Satellite (UARS)
Solar Optical Telescope (SOT)

B. Begin development of the International Solar Terrestrial Physics (ISTP) program as the highest priority new start.

C. Plan for later major free-flying missions and carry out the technology development they require. These are:
Solar Probe (target launch date 1995).
Solar Polar Orbiter (target launch date 2000).

D. Launch an average of one solar-and-space physics Explorer satellite per year, beginning in the early 1990's.

E. Enhance current Shuttle/Spacelab programs in this discipline, especially in the field of active experiments/space plasma lab type missions, Spacelab-Sunlab instruments, and Spartans that make optimum use of Shuttle capabilities.

F. Develop facility-class instrumentation for first use on the Shuttle and later evolution into a Solar Terrestrial Observatory and an Advanced Solar Observatory on Space Station/Platform and develop mission operations and data analysis plans for these observatories.

G. Augment the solar terrestrial theory program by FY 1990.

H. Support a computer modeling program, including access to supercomputers as recommended by the Academy's Physics Survey Committee report.

I. Strengthen the research and analysis program.

J. Maintain a stable suborbital program for flexible, quick-turnaround science objectives in upper atmospheric and space plasma physics.

K. Continue mission operations and data analysis funding for existing spacecraft, in particular for studies of the heliosphere (IMP-8, Pioneer 10 and 11, Voyager 1 and 2), the terrestrial environment (ISEE 1 and 2, Dynamics Explorer, SME, and AMPTE), and the Sun (SMM).

The Committee feels that the program outlined above will result in a healthy and vigorous research effort and will allow the United States to maintain its leadership at a time of increased international attention to this area of science. Our national interest will be well served by increasing our efforts to understand our space environment. The program envisioned in this report is scientifically challenging, technologically achievable, and reflects the natural evolution of a scientific field that was the first to be explored when the nation ventured into space. The implementation plan proposes a constant yearly funding level and distributes the new starts over a number of years.

PRIORITIES FOR IMPLEMENTATION

I will not comment on each of the items in the preceding list except for two: First, the Solar Optical Telescope is deemed by the scientific community to be of essential importance to future progress in solar physics. It is fair to say that delays in implementation of this program will retard progress in this vital field of research for years to come. I do, therefore, urge that the Congress should appropriate the necessary funds for orderly development of this most vital instrument. Secondly, I would like to emphasize that the highest priority new start in the Committee's judgement is development of the International Solar Terrestrial Physics program in the upcoming budget cycle. This unique international program, designed to investigate the chain of interaction linking activity on the Sun through the interplanetary medium to the Earth's magnetosphere and atmosphere is an undertaking between the United States and its principal allies in Europe and Japan where each participant contributes a substantial element in an overall program that would not be affordable otherwise by any one of the three participants. Not only is the science content of this program of the highest caliber but, in addition, it may serve as an example for future collaboration on programs which can be implemented on an international scale, with consequent savings in cost and time for all parties concerned. In addition, this program would serve to reestablish our credibility with our European allies, which was severely tested following the cancellation of the United States part of the International Solar Polar Mission four years ago.

The overall priorities and the program described by the CSSP is evident in the accompanying chart which covers the time from about FY86 through 2000. As is evident from the chart the overall program (at the level of about $300 million dollars in FY86) can be accommodated under a more-or-less constant funding level in the range of $300 to $400 million per year through the end of the century. The recommended program includes not only intensive, well-defined, missions which address detailed science objectives as is appropriate for a mature science, but also exploratory missions such as the Solar Probe spacecraft which is intended to approach and explore the vicinity of the Sun to within a distance of approximately two million kilometers above the Sun's surface. The chart also suggests that the National Aeronautics and Space Administration establish a line item for instrument development of facility-class instruments, to be flown on the Space Station and its accompanying Platforms so as to assure adequate science activity when the Space Station attains its initial operational capability. This recommendation is intended to forestall the problems which arose during the development of the Shuttle, when inadequate resources precluded the development of appropriate instrumentation to be flown on the early Shuttle missions. Further, the program envisions a line item in the budget which will be exclusively devoted to mission operations and data analysis aspects of the Space Station and its associated Platforms, in recognition of the fact that large facility-class instruments require important resources to be devoted to planning and operational activities during the implementation phase.

THE ORGANIZATIONAL ISSUE

As is evident from the recommendations and the accompanying chart, the program for Solar System Space Physics is broad-based and multifaceted. As such, the CSSP recognized that an increased level of organization is necessary to administer it in an effective and timely manner. Currently, elements of the Solar System Space Physics program reside within four separate Divisions within the NASA Headquarters Office of Space Science and Applications. It is the concensus of the scientific community, as reflected in the recommendations of the Committee's report, that for such a broad program to be effective, it is necessary that it be administered from a central organizational entity. Such an entity was in existence within NASA prior to reorganization in 1980 under the name of Solar-Terrestrial Physics Division. Thus, one of the recommendations of the CSSP is that NASA should consider whether the current management structure is best suited to implement the proposed program. It is my hope, Mr. Chairman, that Congress, through the Subcommittee on Space Science and Applications will offer some guidance to NASA on this issue.

IMPORTANCE OF SOLAR SYSTEM SPACE RESEARCH

Let me now summarize by stating the basic goals of Solar System Space research which are as follows:

1. To understand the physics of the Sun; the heliosphere; and the magnetospheres, ionospheres, and upper atmospheres of the Earth, other planets, and comets.

2. To study the interactive processes that generate solar variations and link these to the Earth, because such processes reveal basic physical mechanisms and have useful applications in many areas of human endeavor.

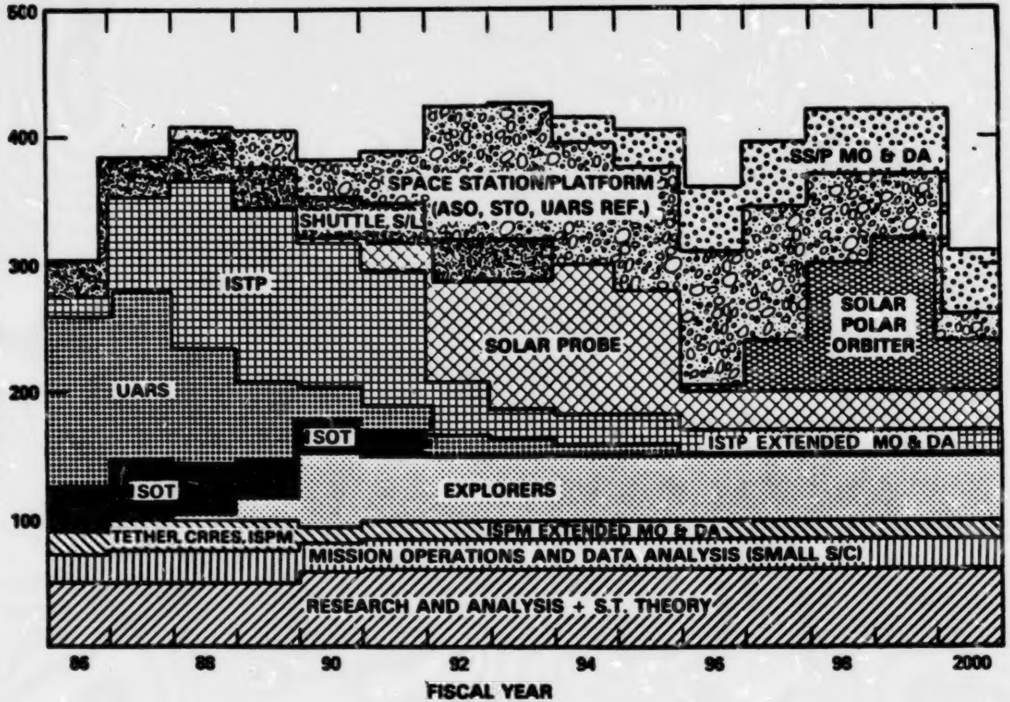
It is essential to recognize the enduring nature of these research goals that, in turn, dictate a commitment to long-term funding, as this report suggests. First, there are the basic physics/astrophysics connections: Because of its proximity, the Sun is the only star whose interior structure and atmospheric physics can be studied at high resolution, thereby providing unique information about physical processes important to all stars. Magnetospheres, the magnetized plasma atmospheres of the Earth and the planets are now known to exist around pulsars, radio galaxies, and accretion stars, among others. The study of plasma processes regulating the structure and dynamics of the planets' magnetospheres has contributed significantly to the development of basic plasma physics.

Second, there are important connections between solar and space physics and Earth system studies. Variation in solar luminosity and/or irradiance may well affect the Earth's weather and climate; stratosphere and mesosphere ozone responses to incoming charged particles have been observed and documented; variations in solar output of radiation and particles have substantial effects on the magnetosphere, ionosphere, and upper atmosphere.

Finally, the understanding of the near-Earth space environment is not only a basic research enterprise, but also has extremely important practical aspects. Space is being used increasingly for many different scientific, commercial, and national security purposes. Well-known examples include communications and surveillance satellites, and such scientific platforms as the Hubble Space Telescope and the Space Station. These space vehicles must funtion continuously in the near-Earth environment, subject to the dynamic variations of the Sun, of the magnetosphere, and the upper atmosphere. In addition to the effects of the dynamics on the electronic and power systems of spacecraft, these aspects of the near-Earth environment become particularly important in view of the planned long-term presence of man in space. The complement of programs outlined in this report will allow us to model the global geospace environment and will thus allow us to develop a global predictive capability. This in turn, should permit substantial improvements in our abilities to operate all space-based systems in the near-Earth region.

Finally, Mr. Chairman and Members of the Subcommittee, let me say that, on behalf of the scientific community of Solar Systems Space Physics, I am most appreciative of the opportunity to review the status and future directions of this research area, the first of the space science disciplines to be investigated as the Nation ventured into space.

FUNDS REQUIRED FOR SOLAR AND SPACE PHYSICS



Mr. NELSON. Dr. Lanzerotti.

STATEMENT OF DR. L.J. LANZEROTTI, AT&T BELL LABORATORIES, MURRAY HILL, NJ

Dr. LANZEROTTI. Well, I'd like to continue a little bit with what Tom was saying and depart a fair amount from my text and relate a few anecdotes.

Actually, the human use of technology has not been very long. Actually, about the time that my great grandmother lived was the first use of modern technology other than a lightning rod, which was of course invented by our statesman Benjamin Franklin to take care of the natural environment.

But the first use of the telegraph was probably the first use of modern technology. They installed those telegraphs in England and they saw all these funny signals that ran around on the telegraph line. And those funny signals were caused by changes in the Earth's ionosphere, which wasn't understood.

There was a huge industry in the electrical technology industry in the latter half of the 19th century. You can read those old engineering journals and you will see that everybody—a lot of the engineers, the telegraph engineers, were devoting an enormous amount of time trying to understand the fluctuations of the telegraph circuits.

In 1859 the telegraph lines running from Boston—out of Boston—were run without any power whatsoever, because of the huge aurora that was going on. As a matter of fact, they saw fluctuations in those telegraph currents during the daytime. That's how they first—that's how people first knew that the aurora existed in the daytime in the Earth's ionosphere.

In 1972 a line running—a telephone line run by my company running from Illinois out into Iowa was totally disrupted for half an hour, completely out of service, because of a large geomagnetic storm. In 1972 my grandmother was still living. That shows you the connection—the short length of time that we've been involved with technology in the United States, in the world.

I think the thing that I want to leave with you today is that, as the sophistication of the technology that our Nation places into space or on Earth continues to increase—like that land line out in Illinois—that's a lot more sophisticated than that telegraph line running out of Boston, which was run on zinc batteries—so must increase the sophistication of our basic knowledge of this environment, and that's documented by many anecdotes that I can relate to you.

The radiation effect on astronauts—if we go to Mars, we'd better know when there's going to be a solar flare event. I'm not going to be an alarmist. I think that there are ways of getting around the problem, but it's interesting to point out that that solar event that caused that outage of a land line in Illinois in 1972 occurred after the last landing of the last astronaut on the Moon. It's good that astronaut was not—those astronauts were not in space at the time, because we had not planned for that kind of an event and that kind of eventuality.

The models of trap radiation that presently exist—a very mundane subject—none of us work on that anymore because that is not in the frontier of science. These models of trap radiation are insufficient for using the newest generations of large-scale integrated electronics on spacecraft. I mean, modern electronics are capable of spectacularly efficient performance—spectacular—these low-powered things. That's why we have all these calculators.

But their sensitivity to radiation sometimes means that they can't be used, and we need much more sophisticated models of the environment.

Spacecraft charging—spacecraft have funny surfaces and they charge to various potentials and they zap—an electric field zaps across in a voltage just like a lightning bolt, and it ruins the spacecraft. The DOD spacecraft suffered outages, and communications spacecraft.

One of the reasons why we put spacecraft into space to do communications was to get away from this ionosphere propagation. I go back and look at the old Bell System Technical Journals and I see a lot of papers looking at the variation of transmissions across the Atlantic at 15 kilohertz and how that varied, and 30 kilohertz, and of megahertz, and how that varied with sunspot activity.

We've gotten much more sophisticated. We put a satellite up there. The first thing we discovered was there were funny fluctuations on that satellite signal called scintillations. We don't understand what causes those scintillations today. We just know that you don't broadcast to Hong Kong at certain times of the day at certain frequencies or you're going to have funny signals, fluctuations on your signals, and if you're sending data down to Hong Kong from your satellite, you're in a lot of trouble at that time of day.

So we need to understand our space environment from the fundamental physics point of view. I don't get up every day—the company I happen to work for happens to be motivated the same way Jim Van Allen is motivated. They allow me to get up every day and worry about fundamental science, but it turns out that fundamental science pays off in the understanding of our space environment; it pays off in our understanding and allowing us to have more sophisticated technologies to utilize the environment and to be able to utilize the environment with these sophisticated technologies.

And the past history from the time of my great grandmother to now shows that we'd better understand our environment.

Let me say one last thing: the space environment is a little bit like the weather environment down on Earth. You go back 100 years ago we had a thermometer in Philadelphia and we had a thermometer—and I've looked at the time history of the temperature in Philadelphia and New Haven—and there was the other one I looked at, Baltimore I think. There are some interesting time histories. And probably you go out in Chicago and it's the same thing.

But now we need a lot more than that. We have to know where Hurricane Gloria is going. When we start putting men in space, particularly in polar orbit or on the way to Mars, we're going to have to know a lot more about the space environment—multipoint measurements of the temperature, equivalent temperature of that particular environment.

The United States has a space forecast center run by NOAA and the Air Force out in Colorado. It's utilized by industry, surprisingly enough; it's utilized by the military; it's utilized by NASA. I think it's only a step in the direction we're probably going to see in the next 50 years, because we have to know a lot more about the environment.

So even though a prime motivation for doing solar and space physics is not just applications, because we don't know what the applications are going to be, but it turns out that, just like the invention of the transistor from fundamental solid-state physics, doing fundamental studies of our near-Earth and solar environment are going to allow us to use it and place sophisticated technology in it.

[The prepared statement of Dr. Lanzerotti follows:]

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Statement of

L. J. Lanzerotti

AT&T Bell Laboratories

to

U.S. House of Representatives

Subcommittee on Space Science and Applications

October 8, 1985

Mr. Chairman, Members of the Subcommittee, Ladies and Gentlemen.

I wish to address today, by way of examples, some of the more practical reasons for the Nation's conduct of research in solar-terrestrial science. This area of science, as discussed earlier by my colleagues, includes studies of the sun and of the near-earth space environment. This near-earth Environment includes the ionized high upper atmosphere (called the ionosphere), the magnetosphere, and the interplanetary medium.

Strong research activity in the near-Earth space can be justified on several grounds. National resources allocated to this area of science must be based upon assessments of all aspects of the research activity. First, the research can be justified purely on its own scientific merits, as an activity having significant intellectual importance in advancing our understanding of the solar system and the universe. After all, it is often said that 99 percent of the universe exists in the plasma state, so that basic understanding of the plasma environment in which the Earth is bathed would seem to be of reasonably high scientific priority.

Second, research in near-Earth space can be justified on the basis of its impacts upon laboratory physics — particularly the area of plasma fusion physics. Many of the linear and non-linear plasma instability problems of laboratory fusion machines are found in the magnetosphere, and vice versa. Laboratory and space plasma experiments are complementary, exploring quite different ranges of physical parameter space. A particular type of plasma shock wave, the collisionless shock, was first discovered in space in front of the Earth and then later studied in the laboratory. Understanding of the loss of trapped particles from the Earth's radiation belts and from plasma fusion mirror machines has advanced in parallel. Spacecraft diagnostics in space plasmas do not in general perturb the system whereas placing diagnostic probes in a laboratory machine often changes the physical conditions.

Finally, and perhaps of most importance, near-Earth space research, while seeking fundamental understanding of nature, can be justified by its ultimate relevance to societal and technological problems and systems. I would like to devote some attention to this topic, because I am sure that such a justification is important to the American public who provides, with tax dollars, the opportunities for space research.

I do not have time to discuss in even a cursory fashion all possible areas of practical importance that arise from solar-terrestrial research. Nevertheless, one theme that I would like to leave with you is that as the sophistication of the technology that our nation employs on the Earth and in near-Earth space increases, so must increase the sophistication of our basic knowledge of the environment (and its temporal changes). This theme can be well documented from contemporary examples. As an illustration, when synchronous-orbiting communications satellites were conceived in the 1940's by Arthur Clark, he could not have anticipated that energetic electrons and ions trapped in the Earth's magnetic field would be an important factor that would have to be taken into account by spacecraft designers. The radiation severly degrades the performance of solar cells used to power the spacecraft, as well as satellite thermal control surfaces and exterior coatings. The models of the trapped radiation that presently exist are often found to be insufficient for the optimum use of the newest generations of large-scale integrated electronics. Although modern electronics are capable of spectacularly efficient performance, their sensitivity to radiation means that considerably more accurate models of the space environment are necessary. Such models would ideally include time dependencies of the radiation; hence the necessity of studying, and hopefully understanding, the basic physical processes of particle containment in the magnetosphere is clear.

Another spacecraft surprise was the discovery that, because nearly all satellites have exteriors consisting of various insulating, as well as conducting, materials, different portions of a spacecraft can become electrically charged by the particles in space. If these potential differences become large enough, electrical discharges can occur across the surface, and even to the interior, likely causes of some past spacecraft failures and of some continuing transmission anomalies on a number of operational communications and DOD spacecraft. The Air Force launched a research satellite, Scatha, to specifically address this problem. It is largely the temporal and spatial dependencies of the particle environment in space that determine the charging characteristics of a satellite, so again basic physical understanding of nature is required.

Much ionosphere research has been motivated in the past by the design requirements for high-frequency radio communications. Our country does not now depend upon such communications as extensively as a number of other nations. However, the shift to transmissions in the higher (microwave) frequency range using communications satellites did not totally eliminate the environmental problems. First and foremost, of course, are the problems associated with the weather. Rainfall produces severe propagation anomalies and requires diversity of ground stations. However, a major surprise was the discovery that in the equatorial regions of the Earth the amplitudes of satellite signals often suffer unexpected fluctuations (called "scintillations"). These scintillations appear to be produced by special plasma physics processes that occur at ~200—300 km altitude in the low latitude regions of the Earth where the Earth's magnetic field is nearly parallel to the layers of plasma particles. Continuing, basic research into this phenomena seeks to undersand the reasons for scintillation occurrence and the distances over which such occurrences are found.

Operational and design requirements have led to the establishment of space-environment forecast centers, notably those operated domestically by the National Oceanic and Atmospheric Administration (NOAA) at Boulder, Colo., and the U.S. Air Force Global Weather Central at Offutt Air Force Base, Omaha, Nebr. Short-range forecasts in real time are provided, as well as longer-range predictions of a variety of environmental changes and resultant effects. The services of these centers span the entire gamut of solar-terrestrial physics and phenomenology, ranging from predictions of solar flares to ionospheric effects and magnetic fluctuations at Earth's surface. Scientists, commercial customers and agencies of the government receive these forecasts.

The utilization of space based on the availability of the Space Shuttle and the anticipated space station suggests, moreover, that new demands will be placed on our ability to forecast the space environment in which such spacecraft fly. This will be particularly true for Shuttle and space platform flights that will cross the polar regions. The complexity of modern spacecraft and the very fact that humans will be flying with increasing frequency will add yet additional requirements for warnings and alerts. While in many instances space systems can be made relatively immune to environmental effects through careful engineering design, that immunity will frequently be purchased only at considerable cost. The cost tradeoffs depend on the presumed accurate knowledge of the space environment and the variability of the environment. Just as meteorological research leads to models of weather patterns, essential for the safety and efficiency of civilian air transport, so are models of the space environment and solar activity mandatory for space transportation.

A growing interest concerns applying our improved ability to forecast disturbances in space to forecasting their effects on ground-based systems. For example, it has been known for many years that geomagnetic storms cause difficulties, including total outages, in cable communications systems and can severely disrupt power distribution systems. More recently, it has been observed that geomagnetic activity can induce large electrical currents in oil and gas pipelines, making difficult corrosion protection monitoring. Utilities and communication corporations are thus very interested in regional forecasts of the time rate of change of the ground-level magnetic field that might alert operators to unusual electrical current flows.

Geophysical explorations for minerals and petroleum now typically use airborne, marine and ground-based magnetic sensors sensitive to about one part in 10,000 (or better) of the Earth's background magnetic field. At high latitudes in the auroral zones, fluctuations in the background field produced by ionosphere currents and waves can be tens or hundreds of times larger than sensitivities of these instruments. Sophisticated analysis techniques are required to eliminate the unwanted natural fluctuations in order to search for resource deposits. Predictive capabilities for determining times of solar activity which greatly increases magnetic disturbances would be a significant advance since the large expenses of deploying a survey party in often remote locales during such a period could be avoided. These capabilities should eventually result from basic research that provided a deeper understanding of solar and auroral phenomena. Presently, since such predictive capabilities are almost nonexistent, a period of several weeks of excessive magnetic activity is one of the hazards that those conducting exploration surveys near the auroral zones must face.

In addition to the above representative recent problems which our space environment has found to pose for new technologies, we should ask ourselves the two following questions:

(a) What possible long-range impacts might solar-terrestrial activity have on society; and (b) is society affecting our environment in ways which may be detrimental in the long term? In order to properly assess the allocations of resources among various scientific disciplines, the NASA must address these questions as well as the more firmly based relevancy issues I discussed earlier.

In terms of the first question, much has been written on the possible relationships, on the time scale of decades, between solar activity (as reflected in the long-term "spottedness" of the Sun) and climate and, on the time scale of days or weeks, between solar activity, interplanetary conditions, magnetic disturbances, and hemispheric weather patterns. While strong evidence appears to exist for such relationships, the absence as yet of specific mechanisms and models for producing such cause-effect relationships prevents me as a scientist from being able to draw definite conclusions. Surely, however, knowing what we now do of the variability of the Sun over the sunspot cycle and of the variability of the near-Earth environment, this is a subject that deserves serious work.

In terms of the second question, a major discovery in the last few years has been the observation that high harmonics of power from the 60 Hz and 50 Hz power grids in industrial North American and European nations are radiated into the magnetosphere and, by some unknown physical process, appear to be greatly amplified. This raises such issues as (a) how increasing industrialization around the world will eventually affect the magnetosphere and ionosphere; (b) whether such changes will be detrimental directly or indirectly to society and/or technology; and (c) whether there is any cause for concern at all? Again, scientific discussions of the issues and basic research are required.

In summary, I believe I can unequivocally claim that studies of the near-Earth space environment have intrinsic basic scientific importance as well as importance for many modern technologies. I appreciate the opportunity of appearing before you; I hope I have been successful in informing you of the importance of our country's continuing national interest in solar-terrestrial research.

Mr. **Nelson**. Thank you very much. Most stimulating.

OK. Let me give my colleagues a chance to ask. I assume it was Mrs. Meyers, Mr. Smith, and then Mr. Volkmer, and then Mr. Slaughter. OK. Let's go in that order.

Mrs. **Meyers**. No questions, Mr. Chairman.

Mr. **Smith**. I'm going to pass at this time, too, other than to say that it's just fascinating really.

Mr. **Nelson**. Mr. Volkmer.

Mr. **Volkmer**. Dr. Van Allen, you have touched on something in your statement—I have reviewed your statement—that I think many people are concerned about, and that is the question of continued funding levels that we presently have for the science aspect of the NASA programs with the space station coming on.

You basically went through that with the shuttle, did you not?

Dr. **Van Allen**. Yes, sir; I think that's a fair statement—fair comparison.

Mr. **Volkmer**. And you mentioned you'd like to see funding up to 50 percent. Some of us are hoping that we can hold it at around 20 percent—that we don't get back down to about 9 percent. Do you understand?

Dr. **Van Allen**. Yes.

Mr. **Volkmer**. Now, also, as I understand, if you had your druthers, you'd much prefer the smaller type of satellites, experiments, rather than like the Hubble telescope and those types; is that correct?

Dr. **Van Allen**. Well, not exactly, no. I'm participating in the Galileo mission, for example. That's one of the so-called billion dollar missions. It's a very powerful and worthy mission. I think the Hubble telescope is in the same general category, in my judgment—very worthy and extremely important mission, as have been the Voyager missions.

I'm participating—I was on the—I was chairman, in fact, of the working group that developed the Galileo mission; also that developed the Voyager missions. That's quite a while ago now—in 1972, I think, where they developed the Voyager missions, which eventually were launched and are doing great. So I have a very deep dedication to major missions, but I just hope that they are not the only thing we have. I hope we have room left for some rapid response, flexible missions in the sounding rocket-small satellite program. I think they have a very important function.

As you know, I make my career in a university. I'm very sensitive to the participation of my young colleagues, the students and younger colleagues, and I perhaps represent that point of view more than anyone else here today, I should think.

Mr. **Volkmer**. Are there any other planetary missions that are, what some of us feel, maybe look-back-at-Earth-type missions that you would like to see? We've had mentioned here in the statements the ISTP mission with the international——

Dr. **Van Allen**. Yes.

Mr. **Volkmer**. But we've got others on line also, on both areas. Do you have any preferences in regard to any of those?

Dr. **Van Allen**. Well, my preferences in that area definitely run toward what I would call major missions. My favorite major mission in the planetary area would be one to go to Saturn, orbit

Saturn, and investigate Titan. I think that has the greatest scientific prospect of any not yet undertaken planetary mission.

That is not to be considered a low-cost mission, I might say.

Mr. **Volkmer**. No; that—I agree with you; that's not going to be a low-cost mission. It's going to be big bucks.

Dr. Lanzerotti, do you have any things that in the space physics or——

Dr. **Lanzerotti**. Well, I'm here primarily as a scientist who primarily does work in space physics, solar terrestrial research, but also the plasma environments of the planets.

And my—I think that the Nation should proceed as expeditiously as all factors allow with this International Solar-Terrestrial Physics Program. I think it's of basic or fundamental scientific importance. I think it has feedback to laboratory plasma physics as well. The coupling between solar and space physics and laboratory plasma physics is really very, very close, and many concepts proceed in parallel between the space and the laboratory.

And it also has this international participation. It was fostered by NASA basically as an international program. And to now drop the ball after being the leader and fostering this program doesn't appear to me to be the way to proceed or to encourage future foreign collaboration, as Mr. Nelson was discussing earlier.

So in terms of solar and space physics, that would be my preference; no question.

Mr. **Volkmer**. All right. Dr. Krimigis.

Dr. **Krimigis**. Needless to say, I concur with Dr. Lanzerotti. As I mentioned earlier, I believe that when your subcommittee goes to Moscow, having a planned program in place is the best way to begin building these methods of collaboration with the Soviet Union, and particularly if we intend to go that route, before we undertake any more extensive negotiation such as some of the missions that were discussed by Carl Sagan earlier this afternoon.

We have to learn how to work with scientists from the Soviet Union, and it would appear to be exceedingly difficult to undertake a major program, even the one on an unmanned sample return, let alone a manned program, before we have learned how to do sort of smaller programs which are relatively decoupled from each other in terms of the technology and the kinds of concerns that exist today vis-a-vis the Soviet Union.

I believe the International Solar Terrestrial Program will be very useful in that regard.

Mr. **Volkmer**. Well, we kind of missed the bet on Halley's comet, didn't we?

Dr. **Krimigis**. We certainly did.

Mr. **Volkmer**. And I hope we can learn from that and a few others that we've had in the past where we had done it.

So, thank you, Mr. Chairman. I appreciate the testimony of the witnesses.

Mr. **Nelson**. Mr. Slaughter.

Mr. **Slaughter**. No questions.

Mr. **Nelson**. Dr. Van Allen, I'm interested—you talked about the sounding rockets. Your concern is that money is going out of the budget for that and put it elsewhere?

Dr. **Van Allen**. Yes. Yes; that's correct.

Mr. **Nelson**. Do you want to elaborate for us?

Dr. **Van Allen**. Well, of course, many of us got our starts with sounding rockets. I started with utilizing the refurbished captured German V-2 rockets at White Sands in 1946. That was, in my judgment, the beginning of the space epoch. However, that was before most people had ever heard that term used.

And then I oversaw the development of the Aerobee rocket, which has been enormously successful, and as of February of this year, there had been 1,058 Aerobee rockets fired. And I've always wished that I had gotten a commission on those, but I didn't.

Now those were used for all sorts of very fine scientific purposes—atmospheric physics, solar physics, astronomical missions, discovery of x rays from the Sun, discovery of x rays from the galaxy, but especially concentrated in the atmospheric era. Now that's a kind of thing—and they're still being actively used.

They were developed, the first launch was 1947. It was sort of the model T of upper atmospheric research. They were still being used as of 1985. Now they have been discontinued in favor of a lower cost solid fuel rocket, which is very reasonable, but, as I say, there are over 1,000—I think 1,058—of them launched. Now that, to me, illustrates an efficient use of technology in the forwarding of science.

Now that continues to be, more or less, the case. Sounding rockets have obviously a very brief excursion into the region above the atmosphere, through the atmosphere. The time duration is measured in minutes, seconds and minutes, and tens of minutes.

Nonetheless, they attack specific questions. Dr. Dupree showed a beautiful example in the caucus this morning of the high resolution spectra on a particular star which is obtained by a sounding rocket. So we have many specific missions we can attack with sounding rockets now, still as of this date.

Now the satellites have to fly above the atmosphere; otherwise, they are—or at least above the, you might call it, sensible level of the atmosphere. Otherwise, they run down too fast. So sounding rockets bridge that gap between balloons, which is about 30 kilometers, and the range of satellites, which is more like 300 or 400 kilometers altitude.

Sounding rockets are relatively inexpensive. They're relatively rapid. The turnaround—we can probably in the course of a year build an experiment for a sounding rocket and have it flown and the data acquired.

In the space flight business these days, it more resembles a decade. I think that's one of the greatest barriers we have to follow-up work—is the enormous delay of major missions these days. So the sounding rocket fills that gap. They fit in with the university style work.

In my own personal experience, a graduate student can help devise the experiment, help with the field work and conducting it, get the data back, analyze and interpret the results, and have a Ph.D. thesis on the basis of his work—all within a 3-year period. That is totally out of the question with major missions these days. It's a—it's one of the most precious experiences that I think any student can have who aspires to this field.

Mr. Nelson. Dr. Krimigis, what institutional improvements do you think that NASA can make on solar terrestrial research?

Dr. Krimigis. As I indicated, the first and foremost is the need for coordination at the top. And that coordination is not readily available when pieces of the program are managed in four different divisions within the agency.

So, even though ideally from the Academy's standpoint, we have stated the major science objectives that need to be implemented, and in principle the agency can then take these objectives and put them together and come out with a program, the fact of the matter is that, unless you have a high-level visibility within the administrative structure, it is very difficult to do an efficient job and acquire the resources that are necessary to implement a coherent program, an efficient program.

So I would believe that first and foremost would be the consolidation of the programs into a division status within the NASA Office of Space Science and Applications. That would be the principal recommendation.

Mr. Nelson. Do you think that this research has adequate flight opportunities today?

Dr. Krimigis. Well, it certainly has not had in the past 5 years or so, and to some extent it's partly due to the fact that, of course, flight opportunities for all disciplines in the past 5 years have not been very good. But it is particularly so in the area of solar and space physics, and it coincides with the disestablishment of the Solar Terrestrial Physics Division that did occur in 1980. I believe that things would have been significantly better had that not occurred.

And, again, it shows the very tight connection between having a well-established set of science objectives and having the administrative structure to support and implement those.

Mr. Nelson. What do you think the impact on science would be if there's a significant delay of this solar optical telescope?

Dr. Krimigis. I'm not—my field is not solar physics, but I have talked to essentially all the leading lights in the field throughout the country and also internationally. And the statement that comes back is that, unless we are able to look at the Sun with the 70-kilometer resolution that the solar optical telescope will allow us to do, there is little progress that will be made in terms of the energy transfer in the solar atmosphere that is necessary to understand a whole host of phenomena, including the flare process and the propagation of energy and the acceleration of the solar wind. And I don't mean to launch into sloganism here.

But I think the basic point is that it ought to be fundamental to our understanding of solar physics, and it's fair to say that solar physics has been at sort of a standstill for the past few years in a fundamental way, and it's likely to continue in that state unless the solar optical telescope is implemented in a timely manner.

Mr. Nelson. Dr. Lanzerotti, do you see any motivation in the private sector to get moving in solar terrestrial research?

Dr. Lanzerotti. I don't see any motivation more than presently is being done. There are aerospace companies that are involved in solar terrestrial research, as I would define it, and there are some of the communication companies in the United States who do a va-

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riety of work, most of which is very practical but does give rise to scientific discoveries and some understanding.

In fact, this scintillation phenomena that I mentioned earlier was discovered by an engineer at the Comsat Corporation. That was not followed up there very much. It was basically followed up by university researchers, but there are small groups in a variety of firms in the United States who do work in this area because they see it either as part of their business purpose or possible future of other research activities.

But most of the research in this area is, indeed, carried out by the Federal Government and by national governments abroad, particularly the predictive capabilities of the environment. The United States is probably leading in that regard, although the Soviet Union has a large effort—if you look at the Soviet literature, they have a very large effort in prediction, some of which is not very good scientifically, but directed toward a prediction, more prediction of the environment for communications and other purposes.

Mr. **Nelson**. A few weeks ago we were up at Bell Labs and visited with Dr. Pensias, and I've been fascinated by the theories of the big bang. And he's the one that put it together, did he not, or he——

Dr. **Lanzerotti**. Well, he and Bob Wilson discovered the 3-degree radiation. I'm glad that he's my vice president. He is a great proponent of fundamental research in an industrial environment.

Mr. **Nelson**. Now would you explain to me how the radiation that we are detecting now is of an origin billions of light years ago?

Dr. **Lanzerotti**. You've placed me in a very difficult situation, particularly sitting in front of George Field.

And, George, are you going to be on this panel? You'll have your chance. [Laughter.]

Mr. **Nelson**. I might ask you the same question Friday if I don't understand it more than I do right now.

Dr. **Lanzerotti**. I'm not sure, but one possible—one way of looking at this is to have a very hot gas confined in this little cup [referring to small paper cup used for water], and to allow that hot gas to cool down because it expands into this room—the analogy is pretty bad because it will collide with this atmosphere and stuff. But basically that gas will expand and cool down, and the—in a very crude sense, the universe started as the size of this cup, and the radiation that existed at that time has—the universe has expanded and the radiation has cooled, so that the present temperature of the universe is 3 degrees Kelvin, which is just below the temperature of liquid helium.

How's that?

[Mr. Field in the audience nods his head in agreement.] [Laughter.]

Dr. **Lanzerotti**. I'm going to take his astronomy course next year. [Laughter.]

Mr. **Nelson**. Yes, Mrs. Meyers?

Mrs. **Meyers**. I get the feeling from you, from all three of you—and it's probably because of your particular focus—maybe you most especially enunciated this, Dr. Van Allen—that you would like to see us maybe reorient our priorities a little, maybe away from the shuttle, away from the space station, and more back to space science, solar and space physics. Can you comment on that? Did I hear you all correctly?

Dr. VAN ALLEN. Well, you certainly heard me correctly, Mrs. Meyers.

I'd like to say there are lots of, what I'd call, incremental approaches to the purpose of the space station, and one of those ideas—not my idea—one of the ideas I thought was rather worthy of consideration—is to use the empty tank off the shuttle to put—actually put that in orbit, cleanse it out, and refit it as sort of a junior grade space station. Now that's vastly less expensive and less difficult than the line that we're taking in the United States at the present time. That's one idea.

Of course, it's easy to recall that the skylab was a kind of junior grade space station at one time, and that eventually reentered the atmosphere. It could have been repropelled and kept at a constant altitude. That's essentially the Soviet approach at the present time with the Soyuz-Salyut combination.

I think it's difficult to make out what important work has been done by the Soviets in their prolonged manned flights, which are something we're aspiring to do now. I'd like to hear someone make a critical discussion of what the yield from that has been. They've had at least, usually a crew of two persons in space more or less continuously for some years, replacing them every 6 to 8 months, on this order. It's not been impressive; I don't think.

One of the principal things that the President has mentioned in the use of the space station is the so-called manufacturing in space. Now that's kind of a catchword these days, but—and he spoke also of manufacturing exotic pharmaceuticals for the cure of human disease and so forth. Now one cannot be 100 percent certain that will not be important, but there's an enormous way to go there. I think this is a very legitimate research field at the present time to inquire into the separation of complex protein molecules from each other, enzymes from others of different molecular weight, and to attempt to grow large crystals. It's a very legitimate research field, but the extrapolation from the embryonic state of this field at the present time to justify any undertaking of the magnitude of the space station is sheer lunacy in my opinion.

Dr. LANZEROTTI. May I follow just a moment? I would like to reemphasize that I don't oppose the space station per se. I tried to indicate, though, that we must be careful. The space station could cause us, if we don't think about it, to reorder our priorities in space science, and we want to make sure that any ordering of priorities is done in an intellectual and well-thought-out manner.

And that's why I said that the space station will facilitate some aspects of planetary science with a large telescope to look continuously at the atmosphere of Jupiter, for example, but we have to make sure that that doesn't preclude the possibility of doing an unmanned spacecraft around one of the planets, the same way we don't want to preclude flying a spacecraft somewhere else in the Earth's environment other than at space station altitudes.

The space station could be enormously important for some aspects of astrophysics. So we want to make sure that we have our scientific priorities, our national scientific priorities, well ordered and not allow the space station to change those unless we desire that.

Mr. Nelson. Any further questions?

Mrs. Meyers. I appreciate those comments because I think in this time of a real dollar shortage it's important to have a discussion of priorities, and I think sometimes we don't state plainly enough just exactly what we mean. We're afraid of stepping on somebody else's program. At any rate, I appreciate your comments.

Dr. Van Allen. That induces my attempt to be clear about this. I think the space station will eat us alive as far as getting on with science is concerned, if it goes ahead as the President now proposes. Is that clear enough? [Laughter.]

Mrs. Meyer. Very clear.

Dr. Van Allen. Mr. Chairman, may I make kind of a delayed—I hope constructive—comment to your inquiry about the Soviet collaboration?

I think the—we have kind of a new high at the present time in the exchange of—advanced exchange of detailed information with respect to the Halley comet encounters. And Professor Sagdeyev was in a group in which I was a part in Pasadena I think about 8 months ago or 6 months ago, something of this order. And he made very strong pledges to make the data available essentially in real time during the Soviet—the two Soviet encounters with Halley's comet. And the Japanese have made a similar pledge, a somewhat less strong pledge, I thought. And the United States will participate, together with the—we know we'll get a good exchange with the European Space Agency. I think that's essentially understood.

But that is what I'd like to put forward as a test of sincerity on the part of the Soviets: Will they really deliver as promised? I think that's a very important test of sincerity which you might like to have on your mind when you go to Moscow.

Mr. Nelson. We will raise that issue.

Dr. Van Allen. Yes.

Mr. Nelson. Thank you very much, gentlemen.

The committee is adjourned.

[Whereupon, at 3:51 p.m., the subcommittee recessed, to reconvene the following day, Oct. 9, 1985, at 1:30 p.m.]

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SPACE SCIENCE: PAST, PRESENT AND FUTURE

WEDNESDAY, OCTOBER 9, 1985

House of Representatives,
Committee on Science and Technology,
Subcommittee on Space Science and Applications,
Washington, DC.

The subcommittee met, pursuant to recess, at 1:39 p.m., in room 2325, Rayburn House Office Building, Hon. Bill Nelson (chairman of the subcommittee) presiding.

Mr. Nelson. Good afternoon.

The meeting will come to order.

We are continuing our hearing on space and Earth science. Yesterday we had this series kick off with a most stimulating discussion. Our witnesses certainly have shown their own personal great expertise in their particular disciplines, but they have been able to come forth with statements that articulate their scientific excitement for the future.

Now, today's panelists are quite qualified and exciting. The first panel, Earth and life sciences, has eminent scientists who have studied how the Earth and its life can be uniquely studied from space.

We're gaining a better perspective of the Earth from the vantage point of space, both literally and figuratively. Sciences of meteorology, biology, and oceanography have all expanded by leaps and bounds just in the short period of time that we have had this opportunity to have a view from space. We're learning more about our planet as a system from space observations as well as the relationship of the Earth as a planet to the universe.

The recent coverage of Hurricane Gloria was evidence of the superb work of our weather satellites. And as a result, loss of life was minimized in what could have been a catastrophic storm, thanks in great part to the advances of space science.

Now, the second panel, composed of experts in the area of planetary science, will examine past successes as well as the future expectations for exploration of our solar system. Dr. Lew Allen, in addition to being the Director of NASA's JPL, is also a vice president of Cal Tech. And joining Dr. Allen on this panel will be Dr. David Morrison, who has recently chaired the Solar System Exploration Committee. We're most interested in hearing all of these witnesses.

I want to turn to my colleague, Mr. Walker, for any comments that he might have.

Mr. Walker. Thank you, Mr. Chairman.

I have no comments. I join with you in welcoming our witnesses, and we look forward to their testimony and look forward to having this as a part of the record upon which we will build in the upcoming authorization cycle.

Thank you.

Mr. **Nelson**. Gentlemen, welcome.

We would like you to proceed in a more conversational and informal way, and we will have your entire testimony inserted as a part of the record, your written testimony. So if you would share with us without reading your written testimony, that is already going to be a part of the record.

So we'll just take each of you in the order in which we're listed right here on the agenda.

So, Dr. Soumi, welcome.

STATEMENT OF DR. VERNER E. SUOMI, PROFESSOR OF PHYSICS, DIRECTOR OF SPACE SCIENCE AND ENGINEERING CENTER, UNIVERSITY OF WISCONSIN, MADISON, WI

Dr. **Suomi**. Thank you, Mr. Chairman, for the opportunity to present my views on how the Earth and its life can be studied from space—although you have already stolen some of my punch lines.

Having been closely involved with the weather satellite program from its very beginning, I can attest to the progress made, as evidenced by the superb satellite coverage of Hurricanes Elena and Gloria. As you said, this was a good example of how technology in the sky which gathers data that was converted to information by gadgets and scientists on the Earth gave rise to an appropriate action by millions of citizens through an effective delivery system and, as you mentioned, few lives were lost.

But this example is only one of the things we can do from space. During the last 25 years of observations from space, meteorology has been one of the leaders—those looking downward rather than upward to give us a new global view. With that, a better scientific understanding has been achieved of the interactions between the atmosphere, the ocean, land surfaces, and ice regions that cover our Earth. We are now able to obtain a global perspective on our total environment.

We cannot study the climate and the atmosphere without involving the ocean. We can do much better. But, as others will say, we need to make a necessary commitment and investment.

Our spacecraft can now carry more weight, use much more power, and, with that, carry more sophisticated instruments. We must make a commitment because some of these observations must continue far into the future. These describe how the atmosphere, ocean, land, ice, snow, and biota are now, as well as their changes in the future. We must also make an investment. That may be the hardest part. We need more sophisticated spacecraft and instruments so we can better observe and understand the interaction between the atmosphere, the ocean, land, and living things in both.

Now I want to give you some specific examples. The first one is my favorite one: Go microwave. I think this is so important that I should have had some bumper stickers printed, so you could use them.

Microwave radiation has the advantage that it can penetrate clouds. During the last hurricane we could not observe what was going on below the clouds. Microwave sounders would correct that. We do have a simple microwave radiometer on our polar orbiters, and they were used, but these satellites pass overhead only twice a day. We need the advanced microwave sounding unit on polar orbiters now, and we need to equip geostationary satellites with a microwave system as well. These would go a long way toward filling gaps in the data which now limit our ability to model the atmosphere's behavior. I call this "stamping out gaposis." The great advantage of the geostationary satellite is that the weather moves, not the satellite.

We must better observe the ocean. The ocean is the great "heat flywheel" of our climate system. We need to know how it moves, because it carries a significant amount of heat, and measure the wind forces which cause it to move. Topex, by measuring the topography of the ocean—finding its hills and valleys, so to speak—can yield the global ocean currents, a key parameter in the redistribution of heat over the Earth for climate.

The scatterometer on NROSS will give the surface winds which drive the ocean currents, and you'll hear more about that, I'm sure. But I would like to emphasize that both are needed at the same time in order that we do the task properly.

We need better observation of rain, especially over the ocean, because the hydrological cycle is the key to climate and bears directly on food production. That means we need evaporation, too. We know how to do this from space, using microwave passive radiation and active radar measurements.

One of the most interesting is the need to know the global chemistry. Our progress in infrared interferometric spectroscopy has been no less than fantastic. Not only are the gadgets good, but the software is even more impressive. These instruments can now be carried on our new spacecraft. One can, for example, measure methane to 1 part in a billion over long path lengths.

And why all this fuss over methane? Interestingly enough, one methane molecule blocks as much heat from the Earth as 10,000 carbon dioxide molecules. All of the optically active trace gases block almost as much as the CO_2, but their rate of growth is much faster. We need to know much more about their sources and sinks, so we can have some understanding of what's happening in the future.

Remote sensing of land surfaces from space, of course, tells us precisely where things are. But, in addition, with sophisticated analysis, which has now been used, we can tell what is happening on the surface in fields of forestry, geology, hydrology, and glaciology. In fact, with accurate satellite tracking, we know more about the shape of the Earth, its gravity field, and the movement of the giant plates which are the source of earthquakes.

I could go on about the existing possibilities of what we can do, but I want to emphasize the next item the most. In order to do this, we need an observing system or EOS. EOS does not mean an Earth observing satellite, but a system. And it does not mean only one satellite, but a group of satellites. And we must not measure our success by the elegant instruments and spacecraft which orbit the Earth. To be sure, these provide much new and highly useful data, but we need the computers, the analysis system, and numerical modeling efforts on the Earth in order to convert this flood of data into information. The apparatus is already here. The people who use it are just as important as the great things we do from space.

As you are aware already, on the tables there are stacks of documents and plans which describe in detail what is possible when we look downward. In this short statement I cannot even provide a satisfactory overview, not to mention giving an adequate description of what is possible.

In a manner of speaking, we have the easy job. We know what is possible. We know that it's very useful, and many of us are anxious to do it. You, on the other hand, have the difficult job. You must take our word that these great things are possible and convince the citizens back home that making the commitment and investment are wise moves. One must convince the hard-pressed farmer in the Midwest that it really makes sense to observe the middle of the equatorial Pacific Ocean, as the recent El Niño has taught us. We must look beyond our city, county, State, or even our country, and press for the global view.

We must stress that our environment's interactions are complex, and we need to know much more. Back home in Madison years ago in the autumn, we burned the fallen leaves. We can't do that any more. It made smoke, but it had a nice odor, and most of the leaves turned into carbon dioxide. What do we do now? We bury them in landfill. Result: we get methane instead. And one home built on this methane leaking field actually blew up. Maybe we have made the problem worse. We just don't know.

The United States has a very fine space program which advanced our knowledge of the stars and planets. These are really look backward, particularly in time for astronomy. We must remember that Earth is a planet, too—a very precious one to each of us. We want to learn about the future well-being of the Earth. As the Europeans are saying in a very fine document, "Look downward, look forward." We ought to join them and others throughout the world to better understand our complex, but precious Earth and how it works.

Thank you, Mr. Chairman.

[The prepared statement of Dr. Soumi follows:]

Testimony by Verner E. Suomi
October 9, 1985

Thank you, Mr. Chairman, for the opportunity to present my views on how the earth and its life can be studied from space.

Having been closely involved with the weather satellite program from its very beginning, I can attest to the progress made, as evidenced by the superb satellite coverage of Elena and Gloria. This was a good example of how technology in the sky which gathers data that was converted to information by gadgets and scientists on the earth gave rise to appropriate action by millions of citizens through an effective delivery system and, as a result, few lives were lost.

But this example is only one of the things we can do from space. During the last 25 years observations from space -- those looking downward rather than outward -- have given us a new global view. With that, a better scientific understanding has been achieved of the interactions between the atmosphere, the ocean, land surfaces, and ice regions that cover our earth. We are now able to obtain a global perspective on our total environment.

We can do much better, but we need to make the necessary commitment and investment. Our spacecraft can now carry more weight, use much more power, and with that, carry more sophisticated instruments. We must make a commitment because some observations must continue far into the future. These describe how the atmosphere, ocean, land ice and snow, and biota are now, as well as their changes in the future. We must also make an investment. We need more sophisticated spacecraft and instruments so we can better observe and understand the interaction between the atmosphere, the ocean, land, and living things in both.

Here are some examples:

1. Go microwave. Microwave radiation has the advantage that it penetrates clouds. During the last hurricane we could not observe what was going on below the clouds. Microwave sounders would correct that. We do have a simple microwave radiometer on our polar orbiters, but they pass overhead only twice a day. We need the Advanced Microwave Sounding unit on polar orbiters now, and we need to equip our geostationary satellites with a microwave system as well. These would go a long way toward filling gaps in the data which now limit our ability to model the atmosphere's behavior. These are all possible with today's technology. The great advantage of a geostationary satellite is that the weather moves, not the satellite.

2. Observe the ocean. The ocean is the great "heat flywheel" of our climate system. We need to know how it moves and measure the wind forces which cause it to move. Topex, by measuring the topography of the ocean (finding its hills and valleys, so to speak), can yield the global ocean currents -- a key parameter in the redistribution of heat over the earth for climate. The

scatterometer on NROSS will give the surface winds which drive the ocean currents. Both are needed at the same time in order to do the task properly.

3. We need better observation of rain, especially over the ocean. The hydrological cycle is the key to climate and bears directly on food production. That means we need evaporation too. We know how to do this from space, using microwave passive radiation and active radar measurements.

4. We need to know the global chemistry. Our progress in infrared interferometric spectroscopy has been no less than fantastic! These instruments can be carried on our new spacecraft. One can, for example, measure methane to one part in a billion over long pathlengths. Why the fuss over methane? One methane molecule blocks as much heat from the earth as 10,000 carbon dioxide molecules. All of the optically active trace gases block almost as much as the CO_2, but their rate of growth is much faster. We need to know more about their sources and sinks.

5. Remote sensing of land surfaces from space of course tells us precisely where things are. But, in addition, with sophisticated analysis, we can tell what is happening on the surface in fields as varied as forestry, geology, hydrology, and glaciology. In fact, with accurate satellite tracking, we know much more about the shape of the earth, its gravity field, and the movement of the giant plates which are the source of earthquakes.

I could go on and on about the existing possibilities of what we can do. But in order to do these things we need an Earth Observing System, or EOS. EOS does not mean an earth observing satellite, but a system. We must not measure our success by the elegant instruments and spacecraft which orbit the earth. To be sure, these provide much new and highly useful data. But we need the computers, analysis systems, and numerical modelling efforts on the earth to convert this flood of data into information. The apparatus here is just as important as the great new things we can do from space.

There are stacks of documents and plans which describe in detail what is now possible when we look downward. In this short statement I cannot even provide a satisfactory overview, not to mention giving an adequate description of what is possible.

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The U.S. has a very fine space program which has advanced our knowledge of the stars and planets. These are really looks backward. We must remember that the earth is a planet too -- a very precious one to each of us. We want to learn about the future wellbeing of the earth. As the Europeans are saying, "Look downward, look forward." We ought to join them and others throughout the world to better understand our complex but precious earth and how it works.

Thank you, Mr. Chairman.

Mr. NELSON. Thank you so much, Dr. Soumi.

Dr. Bretherton—I just had the pleasure of visiting with him in Colorado a couple of weeks ago, and you all were very kind to host us, host our committee with that luncheon. We appreciate it very much. We welcome you to Washington.

STATEMENT OF DR. FRANCIS BRETHERTON, SENIOR SCIENTIST, NATIONAL CENTER FOR ATMOSPHERIC RESEARCH, BOULDER, CO

Dr. BRETHERTON. Well, we were very glad to have you there. I, personally, unfortunately, had an attack of flu that day and was unable to be present, but I heard all about it.

Mr. NELSON. I heard all about you. [Laughter.]

Dr. BRETHERTON. I'd like to address some rather basic questions.

First, why should we study the Earth and why should we study it from space? There are, I think, at least two traditional reasons for studying the Earth: entirely valid and very important to continue. And there's one new reason that I'm going to come to in a minute.

The traditional reasons: simply curiosity. We learn about where we came from, how we came here, what is the world around us.

Another reason is that deep down I think among all our scientists is the feeling someday, somehow what they find out might actually be useful.

We have had enormous successes on this basis. Vern has already alluded to, and you have, too, Mr. Chairman, about—to the weather satellites, to the Landsat Program observing the surface of the Earth. We have recently got the first measurements of the movements of continents, the first direct measurements, that can be compared with the geologic record back over the last hundred—last millions of years. And to my great surprise, we find that, for example, the United States and Australia are moving together at just about the rate they should be—something which is very surprising for the first time we get a new observation, and I'm sure will turn out not to be strictly true when we look more carefully.

We've got new opportunities for understanding the ocean circulation, which will be coming before this subcommittee in the near future for the Topex Program for the NROSS scatterometer and for the geopotential research mission which is aimed at looking at the gravity field of the Earth.

But I'd like to talk a little bit about a broader challenge which affects all of us. Over the last 20 years evidence has been steadily accumulating that the human race is messing up its environment. Now we're all familiar with urban pollution, landfill problems of that type, but I'm talking about something which is much more far-reaching and pervasive than even those very important issues.

The—we have learned to depend upon our climate and the vegetation around us to support our sort of whole way of living. We've learned to expect changes in the weather from day to day, changes somewhat from one season to the next. Every year is not the same. But we do have a pretty strong perception of what is normal, what really to expect, and, if we get a particularly bad snowfall, for example, we think, "OK, a few days it will be gone and life will get back to normal."

But we're doing things. The human race is doing things which we foresee over the next generation or two will change definitions of what we have to expect as normal. Now this has never happened to us in the past. There have been changes that big. The rebound since the last ice age is about the sort of size and effect that we should be thinking about. But we've never experienced it and we've no real idea what impacts that would have.

The basic message that I have is our environment globally is changing in ways we have never experienced before and we do not know what is happening, and the question is: How are we going to find out?

This challenge requires a new perspective on studying the Earth. Traditionally, the Earth sciences have been divided into a number of disciplines. Vern would say his major area of interest was the atmosphere, and Dan would talk about global biology. But we're going to need people who are able to look much more broadly than that and really understand the interactions between the atmosphere and the ocean; the interactions between the climate and the biology; the way the biology is responsible and controls some of the gases like methane and carbon dioxide going back into the atmosphere, which in turn affect our climate; the role of the ice and solid rocks in all of this. These are the sorts of problems we have got to learn to address. And, frankly, we're not set up to do it.

This challenge requires a global view, taking advantage of the unique opportunities we can get from space, but also requiring a systemic study of the processes using every tool at hand, including the in situ observations.

It requires a program of long-term observations, so that we can document the changes that are taking place, at least the most important ones, and we can use that evidence and our understanding of the processes at work to make predictions about what's likely to happen. We may not altogether believe those predictions, but at least they will focus our attention on the things that really matter that are going on around us.

We also require an integrated information system where we don't just go out and measure something, remember it for a few years, and then forget, because our biggest problem here is to be able to come back 20, 40 years from now and look back at what's happening now and saying: are the changes we think have taken place over those 20, 40 years—are they real? Are they just an artifact of the way we took the data or the way we processed it, or have we simply just forgotten?

And, frankly, 99.9 percent of what we learn at this moment is forgotten. It is not remembered in this way. And this, in turn, is going to require fundamental changes in our whole way we archive information and we preserve it for prosperity in useful ways.

In some cases it's going to need new observing techniques. We can't make all the measurements we need to make. In other ways, just as important, it's going to require actually making use of observation systems we routinely have in place now. The weather systems, weather satellites, are an extremely powerful tool for this purpose, but they're almost useless for long-term observations because they're not calibrated properly. We can't track what's going on.

And there's a lot of fundamental things about the data processing that could be done, could be done relatively inexpensively—you know, I'm talking hundreds of thousands of dollars—and are not being done because of the way we set priorities within our system.

We require, for example, that the different Federal agencies work together to achieve these long-term roles. I'm not just talking NASA. The major operational systems we are talking about are, in fact, operated by other agencies—by NOAA and by the Department of Defense. We need to make maximum use of that data, also the data that's available from the commercial sector.

That will require not only NASA making a long-term commitment to the development of systems, to the basic research, but it will require NOAA being in a position to live up to its stated role of making these type of long-term observations and maintaining the archives necessary to do so.

I cannot emphasize this point too strongly because at this moment our Federal structure has essentially seemed—appears from the outside to have cast a mold for NOAA which essentially focuses on short-term services—weather prediction, immediate cost-benefit analyses—and totally ignores these longer term issues of which only, of all the agencies, only NOAA is in the position to do what's needed. We also have a fundamental role for NSF in terms of supporting the basic research into the processes concerned.

Finally, we need the right international developments on this. We have to have the right framework for looking at these things in the international context.

We also need to resolve many of the—we need to resolve exactly how we going to implement the Space Commercialization Act. There are unresolved questions about the role of the Federal Government in developing instrumentation and observing techniques versus the commercial sector.

All of these are really acute concerns in getting a program, a perspective of this sort off the ground. Now those are a terribly daunting list of issues.

On the Earth Systems Sciences Committee we're wrestling with pieces of them—how to put together a set of recommendations for a program that makes sense. But, above all, we can do nothing without the basic commitment at the highest level that our total goal of building a knowledge base and an understanding base for future generations is critically important. It is something which we cannot afford to ignore in spite of the fact that there's a budget deficit of $200 billion, in spite of the fact that the resources are just almost impossible to come by, in spite of the fact that we've got a lot of traditional ways of doing things that dictate short-term priorities and make this type of long-term view seem almost impossible.

What we need most of all is a commitment at the highest level to say we have to do this. And the question I have for you is: Are we going to do it? Is that really my perception of the importance of this problem, one which is shared by a large number of the citizens of this country and the people in the world? Or are we just going to brush this aside and continue down the path we're doing at the moment of basically, yeah, we're looking at the world; we're taking a global view; and we're forgetting it as we learn it and we're not putting the pieces together.

And so 20, 30, 40 years from now our successors will be asking the same questions, but they'll be facing a different environment and they'll be facing a set of challenges, and they won't have time to sort out what's happening.

Thank you.

[The prepared statement of Dr. Bretherton follows:]

PREPARED STATEMENT OF DR. FRANCIS P. BRETHERTON

What is this world on which we live? Why and how did life evolve here? What is our future? These are central questions of Earth Science, for which the focus of attention has long been the Earth, its oceans and atmosphere, the layers of rock and sheets of ice, and the various forms of life. Scientists have long sought the laws that govern the evolution of this planet and the workings of its various components. Space technology has contributed in outstanding ways, such as the network of weather satellites, the direct measurement of the drift of continents, the imaging of the land surface. This effort has to understand Earth processes has led to results of great interest and applications of great practical benefit. It has has been driven by a mixture of motivations: a fascination with our environment, a desire to harness it to our purposes and by the need to adapt our activities to its constraints.

It is important to continue this search vigorously, taking advantage of new scientific insights and observation technologies, both in situ and from space. Signal opportunities in the near future include using altimeters and scatterometers to understand the ocean circulation, ocean color to show the distribution of plankton, and drag free satellites to determine the goid.

However, a new urgency is driving this quest for knowledge. Evidence is accumulating that human activities are changing our global environment, on a global scale and in ways that are poorly understood but with potentially serious consequences for our children and grandchildren. Carbon dioxide and other trace gasses in the atmosphere are increasing steadily, and we expect in due course associated changes in temperature and rainfall patterns. Acid rain and soil erosion are widespread and of increasing concern to biological communities. Changing patterns of vegetation feed back into the global processes which in turn determine the climate. Our present mode of living, our industrial society, is based upon past experience with this environment, as the climate, the soils, the vegetation. We have learned to expect typical weather and seasons, with day to day and year to year fluctuations, but still a definite perception of what is normal. As we move into the future require new definitions of normality will be required, new adaptations, and new recognition of fundamental constraints. Some of these changes may be beneficial, some detrimental, and some of no consequence. If we understand in time what is happening, change may be quite stimulating. If we do not, adjustment will almost certainly be painful.

This challenge requires a new perspective on Earth Science, transcending traditional disciplinary boundaries. It requires understanding how the earth functions as an entire system, concentrating as much on the interactions between the atmosphere, ocean, rocks and ice and the biological subsystems as on the phenomena within them; and revealing the relations between processes that occur on quite different timescales. It requires a global view, exploiting the unique opportunities for observing Earth processes from space in close coordination with other approaches. It requires a program of long term observations, both in situ and from space, to document the changes that do occur, and to provide the basis for computer models to anticipate those that may. It requires an integrated information system, drawing on data from a wide variety of existing operational and research observational systems and those still evolving. It requires in some cases the development of new observing techniques, in other cases cost effective modifications of operational systems maintained for other purposes. It requires a new approach to setting priorities within the federal agencies, including reaffirmations of long term roles and responsibilities. It requires the development of international understandings, on the nature of the challenge and the scientific approach to addressing it, on exchange of data and scientists, and on bilateral arrangements to enhance the program. It requires better working understanding in detail of the roles of government, the academic community and the commercial sector in instrument development and data access. Above all, it requires a commitment, at the highest levels, to a program which will address these concerns in a farseeing coherent manner.

The challenge is great. Much is already happening, thanks to the vision and persistence of concerned individuals, but these scattered haphazard efforts cannot match the sustained commitment required. The scientific community is eager, the capability for major advances is at hand. The requirements are daunting. The organizational problems are severe. Resources are scarce. However, to achieve our goal we must first begin. We owe it to our grandchildren.

Mr. Walker [acting chairman]. Thank you very much. We appreciate your statement.

Dr. Botkin.

STATEMENT OF DR. DANIEL B. BOTKIN, PROFESSOR OF BIOLOGY AND ENVIRONMENTAL STUDIES, UNIVERSITY OF CALIFORNIA, SANTA BARBARA, CA

Dr. Botkin. OK. Let me just tell you a little bit about my specialty. I'm a biologist and my specialty is ecology, which means I study the relationship from living—between living things and their environment, and my work has covered the gamut from studying individual populations of moose and elephants on up to entire forests to global issues such as we're concerned with today.

When I was asked to give testimony here, first I didn't think I could make it at all because I'm on my way to Venice where I'm co-organizer of an international conference which is quite related to this, and so I decided to make time for it. I thought the committee might be a little interested in that.

Mr. Walker. I might say we saw the brochure here.

Dr. Botkin. Oh, you saw the brochure?

Mr. Walker. And I was noticing on the map that there's a "Congress Boat," and I don't know whether that just designates there have been so many junkets over there recently that they've decided to name a boat after us or not. [Laughter.]

Dr. Botkin. That's where all the money that's gone to save Venice is. [Laughter.]

Mr. Walker. We thank you for providing it, though.

Dr. Botkin. You're welcome.

Anyway, this conference, as you can see from that, is "Man's Role in Changing the Global Environment." And it is part of an effort to develop an international global environmental research organization whose purposes are to try to use high technology, including space technology, to find solutions to large-scale environmental issues and to improve our environment, and it's to be a bricks-and-mortar institution where people actually come together from all over the world to do research.

And the purpose of the conference is a first step—first developing that institute and to bring people together to talk about what this ought to be. So it's quite relevant to the issues we're talking about today.

The questions I understand that you'd like me to talk about are: Why study biology from space and what about life can be uniquely studied from space? I'd like to quickly go over my reasons for thinking it is important to study biology from space and then take a little time to amplify each of them.

I do think it's important and useful to study biology from space as a fundamental science and for applications for the large-scale environmental issues that Francis referred to before and which I want to touch on a little bit myself.

My reasons for believing this is important to do are as follows:

First of all, life is a planetary characteristic and, as a planetary characteristic, requires a planetary perspective. Now I'll explain what I mean by that in a minute.

Second, totality of life has and has had for the last several billion years a very strong effect on this planet's surface. The oceans, atmospheres, and sediments are different from what you would find on a lifeless planet. So life has affected the Earth at a planetary level, and you have to study it at a planetary level in order to understand these effects.

That leads to the next point, which is, as my colleagues mentioned already, life and the environment interact and there's an interplay between life in its totality and the atmosphere, oceans, and sediments at a planetary scale. So if you want to understand atmospheric chemistry, climate dynamics, oceanography, geology, you have to understand the interplay with biology. And, also, if you want to understand biology at this level, you must also take into account the interplay with the other disciplines.

Now the fourth reason is that it is necessary to take this global perspective on life in order to deal with large-scale global environmental issues. Now, also, there is a unique perspective and a unique kind of information that we can get about life on the Earth from the perspective of space.

And those are the reasons. Now let me take a few minutes to explain them.

First of all, I said that life is a characteristic of planet, but what do I mean by that? Well, we associate with individuals. Each of us is alive and we think of life as a characteristic of individual organisms, but when you ask the question that is asked in my field, what's necessary to sustain life and how is life sustained, we recognize that something larger than individual organism is required. No individual persists alone; each requires a system of an environment and many other organisms interacting together.

Now the more we think about this, the more we come to the conclusion that this system has to be so large and complex that it's really a planet-sized system. You can't close life up in a bottle and get it to persist for very long. A colleague of mine has done that for 20 years. He's had algae and bacteria surviving, but that's a lot shorter than the 3 billion years that life has persisted on the Earth, and he didn't have any higher organisms, animals, or plants.

So you have this perspective: the more you begin to think about what is necessary to sustain life, then you need a planet, and if you need a planet, then you need a planetary perspective to understand life and its persistence.

The next point I mentioned was that life affects the Earth at a planetary scale—that the atmosphere, oceans, and sediments are very different from what they'd be on a nonliving planet.

Just one example of that, one of the ones I like best, is that all the major iron ore deposits, the economically important iron ore deposits, are the effect of a biological product on the environment. Very briefly, when the first algae in the oceans began to conduct photosynthesis hundreds of millions of years ago, oxygen was emitted into the ocean. This oxidized the iron and converted it from a more soluble to a less soluble form and deposited the iron in the oceans, out as mineral ores, which we now use economically. So if there had never been life hundreds of millions of years ago, there would not be these economically useful ores.

Now that illustrates that even things that are as inert to us as the rocks that we walk on have been affected by biological processes at large scale. And similar stories can be told for other minerals and for the atmosphere and for the oceans.

Now this large-scale effect means also that you have an interplay between life and the environment at large scales. Climate affects life, but there's reasons to believe that life can affect climate, and so the interaction of these large aspects of the Earth must be considered.

Now, then, to return to the point that Francis made, the understanding of life from a planetary perspective is necessary to deal with many large-scale local environmental issues. Repeatedly during the past decades there's been many large-scale issues, including the famine in Africa and the causes of that famine, ozone, acid rain, climatic change. And in every case, it seems to me, we have not been able to provide satisfactory answers. We haven't been able to tell whether there's been change, whether the change was an increase or decrease, what percentage of the change was due to human activities.

Now why have we been so unhelpful as scientists? The reason is that we've lacked a basic science of this Earth life support system, so that we're unable to know what a natural change is, and then we can't determine what a human-induced change is.

What this suggests is that we really are developing a new science. We're developing a new science of the biosphere. There's a need for this new science, and it integrates what we have viewed as traditional disciplines in the past and it builds interfaces between these—the traditional Earth sciences, biology, oceanography, atmospheric sciences, geology, and so forth.

Now this perspective on life on the Earth, this idea of what it takes to sustain for long periods, is also an important basis for those who are interested in studying the origin of life and studying the possibility of life's existence elsewhere in the universe, because unless you understand the condition of the system that allows life to persist, you're not going to be successful in working out how life could originate and then persist or find it elsewhere.

Now, given all this, my—people often ask me, even so, even if you need a planetary perspective, why do you have to use spacecraft or space technology to obtain this perspective? People often think that we know all there is to know about the Earth and its natural resources. You can open up any atlas and find a map of natural resources. For example, you can open up an atlas and find a map of all the vegetation of the Earth which is laid out in bright colors for each type of vegetation with very neat boundaries, and it looks as if we know all there is to know about this, but this is far from true.

Just a quick example: as part of my own research, we needed to know the distribution of the conifer forests, the Christmas spruce fir forests of Canada. We had to know where they were and how abundant they were. And we used traditional maps to begin with. We took the land-based maps, redrew them on the same scale, and compared them. And they didn't agree at all. And depending on how you interpreted those maps, the location of Canada's major forests could differ by 100 percent. Now if any country should know where its forests are, it should be Canada, a developed nation, one of whose major natural resource is forests.

The situation is much worse in developing nations. Many developing nations are only just discovering that their major forest resources have disappeared by using remote sensing imagery. They suddenly discovery that what was on a map isn't there anymore.

Now that's a very quick way of saying that there is no other way except space technology to have accurate and updateable indications of where the biological resources are that are a part of both the basic scientific issues and the applied issues that I've been talking about.

OK. So I'd like to sum up, then, by saying that I believe it's important to study biology from space. It offers the opportunity for a fundamental new and important science, and it's important for our own survival and for an improved environment.

As Francis suggested, this requires—this effort requires not only money, however; it requires a will and a determination. It requires the proper mandate that this be done.

It also requires international cooperation. In order to study life on the earth from a planetary perspective, you must look at life in many different nations. So we have to work with other nations.

So there's a need to promote and support international cooperative programs. Now a number of those—there is some start in that direction. For example, the International Council of Scientific Unions has proposed an International Geosphere-Biosphere Program which would be a start in this direction. The Global Environmental Research Organization, which I mentioned we're trying to begin in Venice, would be a bricks-and-mortar institute that could fill a role in all of this.

Another thing that's required, in addition to dollars, is an interdisciplinary view, which this is the melding of what we have thought of as classic scientific disciplines in the past, but which will need to be brought together to a new scientific discipline.

OK. So, having settled this, what is it that can be done? It seems to me that a great deal can be done. Since x billion of dollars are appropriated annually for the exploration of space—military uses, business, weather, communications, et cetera—I think it would be helpful if this committee could insure that there was an adequate focus on the problems that I've discussed, so that there would be y dollars allocated for the study of the basic and fundamental considerations I've talked about, including the environmental issues.

I'd further suggest that the details of how this could be done, including the points I raised that it's not just dollars but there has to be a will and a determination and a mandate; there has to be international cooperation and an interdisciplinary view—I believe that details of this could be worked out through Dr. Smith of this committee, if he could work together with representatives of NASA and the scientific community.

It's difficult to discuss such complex matters in a brief time. I'd be happy to sit down with Dr. Smith and members of the committee, especially after my return from the conference in Venice, to let you know about the progress in that development and also to talk further with you about questions which might arise as you think through these issues afterward.

Thank you for giving me this opportunity.

[The prepared statement of Dr. Botkin follows:]

TESTIMONY OF
DANIEL B. BOTKIN

Before House of Representatives Subcommittee on Space Science and Applications: House Committee on Science and Technology
October 9, 1985

My name is Daniel B. Botkin. I am Professor of Biology and Environmental Studies at the University of California, Santa Barbara. My specialty is ecology, the study of the relationships between living organisms and their environment. My research during the past 20 years covered many aspects of ecology, from individual endangered species to entire forests and to aspects of our planetary life support system. This research has involved experimental studies in the field as well as the development of new computer models.

I have served on the National Academy of Sciences Space Science Board, as chairman of the Board's Planetary Biology Committee, and on a number of NASA science advisory panels. I have done research under grants from the National Aeronautics and Space Administration concerning the study of the Earth from Space. I have been on the faculty of the Yale School of Forestry and Environmental Studies; a research scientist at the Marine Biological Laboratory, Woods Hole, Mass., and a fellow at the Woodrow Wilson International Institute for Scholars in Washington. I am co-director of the Global Environmental Research Organization, an organization formed by the University of California, and the Universities of Padua and Venice in Italy.

Mr. Chairman, it is my understanding that the questions you would like me to address are: "why should biology be studied from space?" and

"What about life can be uniquely studied from space?" I am happy to discuss these questions today, especially as I am on my way to Venice, Italy, where I am one of the organizers of an international conference, titled "Man's Role in Changing the Global Environment." One of our purposes in organizing this conference is to bring together experts from around the world to discuss the use of high technology, including space technology, to study the Earth from space, to help us improve our environment at a global level, and to help us deal, in a positive fashion, with global environmental problems. The city of Venice asked my university to join it in a project to develop a global environmental research organization. This conference is a first step toward implementing that plan.

I believe that it is useful and important to study biology from space. It is important not only as fundamental science, but for applied issues as well, especially the management of global environmental problems and the management of our biological resources, forests, fisheries, wildlife, agriculture. I believe that space technology, along with advances in other technologies and the scientific advances made during the past decades will allow us to take a new, positive and constructive approach to our environmental issues, so that the environment can be viewed as part of economic development. I believe also that space technology allows us to develop a new and exciting scientific perspective of life on our planet. What we see emerging is a new science--a science of our planet's life support system. This study is important to the other, non-biological Earth sciences. This new science is also important to the study of the origin of life which, in turn, can provide basic understanding for the search for life elsewhere in the universe. Exploring space has been called the new frontier, but exploring life on the Earth from space offers us a new scientific frontier.

The first reason to study biology from space is that sustained life appears to be a planetary phenomenon. To explain this, we need to think about what is required to sustain life. When we think about life, we associate it with individuals. You and I are alive, as are individual plants and animals, and therefore we think of life as a characteristic of individual organisms. But no individual can survive in isolation: every living thing is sustained by a very large and complex system that includes other life and its environment. Often people have thought that one could sustain life just by closing it up in a bottle, just like in my high school science class where the teacher had a bottle with water and a flowering plant sealed with a cork. That plant in its closed bottle might survive several years. The longest anyone has been able to sustain life in such a simple closed system is 20 years, which has been done by Prof. Claire Fulsom of the University of Hawaii. But even his bottles contained relatively simple organisms--algae and bacteria--and no higher plants or animals. The more biologists think about this, the more it seems that to really sustain life for a long time it is necessary to have a very large and complex life-support system, a planet-size system. So, from the viewpoint of sustaining life for a very long time, life seems to be a planetary characteristic, not an individual's characteristic, and to understand the requirements to sustain life, one must study life at a planetary scale.

The second reason is that life has an amazingly large effect on the Earth's entire surface--its atmosphere, oceans, soils and rocks. This effect has occurred throughout the last 3.5 billion years, since life first appeared on the planet, just as it is occurring today. Our understanding of the effect of life on our planet has been greatly increased by recent studies of Mars, Venus, and other objects in our solar system, by rockets sent to these objects, and by observations from the Earth.

Even many of the rocks that we walk on are the product of life, or of life's effect on the environment. For example, all of the major economically important iron ore deposits are the result of life's effect on the environment. When the earliest algae first began to grow and carry out photosynthesis, the algae released oxygen into the ancient oceans. This oxygen changed the iron in the ocean from a more soluble to a less soluble state; the iron was then deposited from the oceans, creating the major iron ore reserves. Without life, these ore deposits would never had been formed.

Scientists now believe that life has affected the formation of many other important minerals. Thus even the rocks that we stand on and mine, rocks that we do not associate with life, are in some important ways affected at a planetary level by life. Life also has major effects on the chemical makeup of the atmosphere. This I believe is a much more familiar story. As every high school science student learns, the fact that our atmosphere is high in oxygen and low in carbon dioxide is because of the same, very long history of photosynthesis on the Earth. But it is also true that life affects many other chemical compounds in our atmosphere, including compounds of nitrogen, sulfur.

The third reason to study biology from space is that life and the environment interact at a planetary scale, and one must understand these interactions to understand the functioning of the oceans, atmosphere, and the solid surface. For example, we know that climate affects life, but scientists now also suspect that life can affect the climate. This is important from the viewpoint of basic scientific research. Scientists in a number of non-biological sciences, such as oceanography, geology, atmospheric sciences must take this planetary view of biology into account in order to understand their own sciences. And we biologists who study the relationship between living things and their environment also must take a planetary perspective on the environment, and involve the findings of these other sciences, in order to understand our own field. We believe that this study of life from a planetary point of view is a new, important and challenging science.

The fourth reason has to do with many large scale environmental problems that we have repeatedly faced over the past several decades, including the ozone problem, acid rain, climate change, and the factors that have led to drought and crop failures, especially those which have become so visible and terrible in Africa in the last several years. We scientists, when asked what should be done to solve these problems, have not been able to give satisfactory answers. When asked whether changes were occurring, we often were not sure. When asked whether some factor such as ozone was increasing or decreasing, we often couldn't say. When asked if the change was natural or human-made, usually we weren't sure, and could not tell what percentage of any change, if we were sure about the change, was due to human activities.

Why have we been so unable to help? The answer is: we lacked the proper basis in fundamental science about our planet's life support system--that complex system that I referred to earlier, which is called the biosphere. In order to deal with these large scale environmental issues, we must develop a new science of the biosphere. This science requires a blending of many disciplines--the blending of biology with geology, oceanography, climatology, atmospheric chemistry, and so forth. It requires a study of the interfaces among the major components of the biosphere--the interfaces among the atmosphere, oceans, soils, rocks, and life. Only by developing this new science can we hope to deal with our large scale environmental issues in an active and positive way.

Mr. Chairman, many people often ask me, even if we need to develop this new scientific perspective, why use space technology? Don't we already know enough about life on the Earth? Indeed, many of the things we read and look at would tend to make us believe that we know all we need to know about life's distribution and abundance on the Earth. But this is not true; in fact it is far from the case. For example, anyone who has an atlas or encyclopedia at home can look up maps of many of the Earth's feature. In any atlas you can find a map of the vegetation of the Earth. When you look at one of these maps, the different kinds of vegetation are shown in bright colors, and the boundaries are sharp, and it looks as if we know all there is to know about the distribution of vegetation on the Earth.

But we really don't. Let me give you an example. As part of our current research we needed to know the location of the conifer forests--the spruce and fir forests--of Canada. These forests are one of Canada's major natural resources, and if any country could be expected to know where a kind of forest was, Canada could be expected to know where its conifer forests were. We took the three best maps of these forests we could find, redrew them on the same scale, and compared them. These maps locate the conifer forests very differently. Using these results, the total area that could be attributed to conifer forests in Canada, based on these three maps, varies by 100%.

These maps were not made from space, they were made on the ground. These maps are made by traveling on the ground, by using low altitude air photos, or by estimating what ought to be there based on climate and other environmental factors.

The situation is worse in other countries, especially in developing countries. In many of these countries experts are only just discovering through the use of satellite imagery that a large percentage of their forests no longer exist.

Experts in these countries had believed, based on conventional, land based methods, that much of their original forests were intact. Conventional, land-based methods of monitoring the extent of our biological resources are not accurate enough, nor fast enough, nor broad--seeing enough to make possible timely, updatable records.

As I have tried to suggest earlier in my statement, information about the distribution and abundance of these biological resources form an essential part of the equations which we must solve in order to deal with many large scale environmental issues. For example, the vegetation of the Earth contains about as much carbon dioxide as exists in the atmosphere, and this vegetation takes up and releases every year about 10% of the atmosphere's total carbon dioxide. The possibility that our burning of fossil fuel may be adding enough carbon dioxide to the atmosphere to change our climate has become a news item periodically during the past years. But whether our effect on the atmosphere will change the climate can only be answered if we better understand to role of life in the uptake and release of this carbon dioxide in our atmosphere.

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The scientific study required to achieve this understanding is best done with a variety of techniques and viewpoints, including a range of measurements made on the ground, with helicopters and light aircraft, balloons and satellites. When we sent a rocket to look for life on Mars, we sent an obiter and a lander. To study the Earth as a planet, we do best to use the same approach--to send up orbiters, and to use our land equipment too. Space provides a unique perspective on life in several ways. Amazing and useful photographs have been taken from spacecraft of the Earth, but, as I am sure the members of this subcommittee are aware, instruments on spacecraft can do much more. Multispectral scanners measure light reflected in specific wavelength bands. These can be related to the amount of vegetation, and to important environmental factors. Radar images, still experimental, may give us greatly improved estimates of biomass and of water. Other experimental devices include narrow wavelength scanners which some scientists believe might someday allows us to do biochemistry from space. The importance of space techniques is not only the scale that one sees with, but the ability for repeated coverage so that changes can be detected; the ability to see in wavengths not possible with the naked eye. These techniques have already proven themselves in the detection of crop diseases, in the prediction of world small grain production. It is now time to apply these to even broader topics as I have outlined here.

Mr. Chairman, in my testimony I have tried to answer the question, why study biology from space. I have given you the following answers:life is sustained by a large, complex planetary system best studied with the help of a viewpoint in space. Life affects the Earth at a planetary scale, and has done so for more than 3 billion years. Life and its environment interact. An understanding of biology from a planetary perspective is necessary to the non-biological earth sciences, including oceanography, and atmospheric sciences. This planetary perspective on biology is important as basic science, and suggests the beginning of a new science of the biosphere. It is important to the attempt to understand the origin of life on our own planet and the possibilities of life's occurrence elsewhere in the universe. This planetary perspective on life is also necessary to deal with large scale environmental issues such as acid rain, ozone, and climate change. To deal with these problems, we must develop a new planetary science, a science that views life from a planetary perspective, and develops a new view of the interfaces between the atmosphere, oceans, sediments, and life.

The view from spacecraft provides unique capabilities for these topics I have just discussed. This unique capability is not only the ability to see very large areas, but to view them repeatedly, and to use many kinds of instruments that let us make measurements not possible with the naked eye. Space technology is needed to provide the information for basic science, and so that we can deal with our large scale environmental issues.

Mr. Chairman, I would like to close by saying that the development of the new science of our planetary life-support system not only requires space technology, it requires international cooperation. At this time, there are several steps that scientists are trying to take to develop this international cooperation, such as the proposal by the International Council of Scientific Unions for an International Geosphere-Biosphere Program. The new research center that we are attempting to develop in Venice could also be an important link in this new science, as could a newly established United Nations global environmental data center in Geneva, called the GRID system, sponsored incidentally by NASA, Canada, and Norway.

Mr. Chairman, I would like to thank you for the opportunity to appear before this committee.

October 8, 1985

Mr. NELSON. Do you have questions?

Mr. WALKER. Thank you, Mr. Chairman. Just a couple.

Dr. Suomi, do you think we can ever consider privatizing meteorological satellites, and what would be the impact on science if we did that?

Dr. SUOMI. I think the impact on the science would really be tragic because meteorological satellites, the operational ones, form the data base for much of the research. Actually, those pictures which were mentioned as being viewed in Florida and which were so important came from the University of Wisconsin. NOAA helped support this work, but I think the interface between research and operations would be ruined with privatization.

Possibly the best person who made a statement on this is John Chancellor. And with the chairman's permission, I would like to read that statement.

Mr. NELSON. Certainly.

Dr. SUOMI. This was on NBC nightly news dated September 3, and I now quote Mr. Chancellor:

> David Brinkley once joked that World War II was the last Federal program that worked. For almost 200 years Americans have argued about how much power the Federal Government should have, arguments which go on today. Some would like to cut Federal power down to the minimum necessary for defending the country and collect just enough tax to pay for it.
>
> Well, the murderous winds of Hurricane Elena remind us of the Federal Government—that the Federal Government has a bigger role to play. Take the National Hurricane Center, the Federal agency which coordinated the warnings about Elena. Those warnings saved hundreds, if not thousands, of lives. The States evacuated people in the path of the storm, but there was no way that State governments could have paid for the weather stations, the satellites in space, and the whole sophisticated system which showed where the storm might go. It's a perfect example of something the Federal Government does that various States can't.
>
> There's a great campaign now in the administration and parts of the Congress to slash Federal programs even more than they have been slashed. The people who produced the Federal deficit now threaten us with it and say that, unless Federal spending is drastically reduced, the deficit will gobble us up. The budget crunch extends to the National Hurricane Center along with everything else. Federal spending is an easy target, but before further deep cuts are made, the politicians might check with the people who benefit from it, and that includes the survivors of Hurricane Elena.

That's the end of the quote.

Mr. NELSON. What was the date of that?

Dr. SUOMI. That was on September 3 on NBC nightly news.

Mr. NELSON. This year?

Dr. SUOMI. This year. I don't know if I've disobeyed any copyright rules, but you have it. [Laughter.]

Mr. NELSON. I doubt it.

Dr. SUOMI. And so I think the role, as Mr. Chancellor put it, is as clear as anything, but I think the real reason of my concern is that the weather satellites which operate for operational purposes form the basis for much of our research.

The rules are very different, and I'm beginning to appreciate them. And many scientists don't really appreciate those different rules.

In operational system, you get more complaints that the forecast is not out on time than if it's wrong, but in science you can take your time but you've got to make it right. [Laughter.]

The two are very different. You have another question on your list for me, "Should the scientific community have any input in defining the instrument packages for meteorological and other satellites?" I surely think they should, because of this enormous impact. And do we have any now? Very little, if any. And so this should be improved.

Thank you, Mr. Chairman.

Mr. WALKER. Well, let me pursue that, though, for just a minute. A lot of what you say is valid, but this committee was out at Goddard Space Flight Center a while back and saw some exciting new things that are being projected as to what we could do with not only data that's now being collected, but with future kinds of things that can be done with computer enhancement of that kind of data. And it seems to me that some of those advances are the kinds of things that would benefit both the operations and the research eventually. And as much as we might want to be sympathetic to the problems of the deficit, the one fact remains about the deficit and that is that it is going to prevent us from doing very much in the future in the Federal Government to expand those activities that are now in place. We may be able to hold harmless on a lot of them, but we're not going to expand them very much.

Now the question becomes, if you privatize some of this and you encourage some competition for the information, isn't there a chance that investors will put the kind of money into new research and into new data and into new equipment that would, in fact, improve the operations, improve the information gathering, and ultimately, then, upgrade research?

Isn't it likely that we'd be more likely to get those kinds of moneys in the future with a technology that we already know exists, through that route, than depending upon the Federal Government, that is at least going to work out its $200 billion deficit problem before it embarks upon a lot of new technology?

Dr. SUOMI. Well, to a certain extent you are correct. As a matter of fact, even in our present system, the private sector does maybe 85 percent of the work. They build the satellites, they launch them, and it's on. They are under Government supervision to be sure, but the work is actually done in the private sector. And some elements of the private sector are very good.

But what I'm trying to say is that in the research area there is little motivation to expend more funds just for research. The private sector is driven by a profit motive, which is a very good motive. We all want our dollars, and so do I. I think that if a mechanism can be devised so that the private sector does what it should do, but that it does not defeat science opportunities, this may have some possibility.

Mr. WALKER. Well, why would there be an assumption that privatization would necessarily defeat research opportunities? Why do we start from that assumption? There's a lot of private work that's going on in many sectors that this full committee deals with where the information is being shared or being used as a part of overall scientific advances. Why can't we assume that that would also happen in the space——

Dr. SUOMI. To a certain extent, that's true, but I would have—the private sector deals with airlines, for example. I can't use my airline window any way I want to. And one has to have a special aircraft to observe the atmosphere for special things.

I would say the same thing is true in the communications satellite area, which has developed greatly. There are limits. And so for some of the things which we're talking in this new field—if the Government were to sponsor the private sector in carrying out these things, then your point may be very well taken, in that they could do these things more efficiently than the present papermill.

As I mentioned, there are stacks of documents which say what we ought to do. And rather than having more stacks, I think we ought to go and do it.

Mr. Walker. Yeah, but the argument—the argument today is that any time we start talking about privatizing meteorological satellites, all these arguments come up and no one talks about the fact that perhaps there could be a mix. I mean, obviously, you're right, you cannot necessarily use your aircraft window for everything you want, but the fact is that you have an aircraft that can take you to places where you can get that special aircraft to do the observation you want. If you depended upon the Government to fly you everywhere you wanted to fly, the chances are that you might not ever get to where you wanted to go. We wouldn't have enough aircraft. We'd be waiting to build aircraft because of deficits.

Dr. Suomi. I won't argue that at all, but I have to say—and in a certain sense you're right. When the first new stationary satellite was developed, which I invented, I didn't build it personally; Hughes Aircraft built it. And without their help, we couldn't have done it. They were very dedicated. But they also had to get paid. Fortunately, the Government had the money to pay it.

I don't think we should debate this issue forever because it's one that deserves a deeper look, and in a short time perhaps we can't do it. I'd be glad to come and visit with you or do anything along these lines and we can continue our debate.

Mr. Walker. Dr. Bretherton, you made mention in your testimony about some problems with the Commercialization of Space Act, I think.

Dr. Bretherton. Yes.

Mr. Walker. And particularly the Federal role in instrumentation, I think. Would you expand upon that a little bit?

Dr. Bretherton. Yes. I believe that the Commercialization of Space Act was fundamentally right, the right way to go, but what I'd like to point out is that, in terms of implementing it, there are some serious unresolved issues associated with the fact that the members of the community—the academic community, the private sector, and the Federal agencies—simply have not yet had time to evolve a set of working relationships or working understandings about who does what.

The sort of problems that arise from the Landsat Program is when Dan wants to use Landsat data to obtain information about where the forests are. Yes, it's there. It's a very, very important research topic, and it will cost about $3,000 an image and there's 15,000 images required to cover the globe. Now that's out of sight in terms of traditional research budgets—the mere cost of acquiring the data through the commercial channels.

Now you talk to the people in EOSAT and they very much want the data to use for that purpose. They are very interested in encouraging the scientific use of that data, for very good reasons. They see that it helps them develop the market and that there's a lot of leverage from their point of view putting private money into that operation.

Now they can't do it by producing a reduced rate for the scientists, but it could be done, for example, by having a trust fund that was contributed to commercially and yet was used to finance the purchase, acquisition of data for scientific research. Now it's just an example of the type of idea that is around.

We get into another problem that relates, for example, to how much—how far should NASA go in developing new instrumentation techniques, the new technology of imaging spectrometers, and where does that start to become in the realm that, say, EOSAT should be putting its money into?

We have no guidelines as to where those interfaces are, and what is desperately needed over the next 3 years or so is a forum in which these issues can be talked out, a forum that has representatives on it—and there is no such thing as a representative of these communities, but members on it—from the academic community, the private sector, and the Federal agencies concerned, and there are several of them.

And so that's one thing I would very much like to draw your attention to, because if we don't get that right, it will derail everything else.

Mr. WALKER. Thank you, Mr. Chairman.

Mr. NELSON. Mrs. Meyers.

Mrs. MEYERS. All three of you seem to have emphasized in one way or another the importance of cooperation in coordinating your information together, getting the Government agencies to work together to share information and to properly document it and keep it so that we don't lose it and forget it.

Who could best set up the plan for that coordination? Who's going to do that? And does that not in itself indicate that somebody is going to surrender a little authority or—that's always a real problem in Government, I think.

Dr. BRETHERTON. As an outsider, I am well aware I'd be extremely foolish to recommend to the Federal Government about how to reorganize itself. I have seen that debate so often and seen it bogged down in all sorts of details that I was totally unaware of so often.

In worrying over these things, it seems to me there are certain ingredients that are necessary if things are to be successful. First of all, I cannot reiterate at the highest levels there has to be a sense of priority. There has to be an overall recognition of the objectives, and that's fundamentally a political process. If there's one underlying message I'd like to give, it's that. If that doesn't happen, nothing is going to work.

The second point is, if you look at the traditional roles of the various agencies that are involved in this area, these are very longstanding, firmly established roles, and I don't think you would mess with them without really upsetting a lot of other things unintentionally.

And in terms of implementing a long-term program of the type we're talking about, we've got to work harmoniously with the other conflicting interests, not in opposition to them. Otherwise, it won't happen.

Now those traditional roles are NASA, which at risk of trying to paraphrase NASA's role has an emphasis on research and developments of new applications of space technology, of demonstration that they're really useful or really important. It has the fundamental scientific role, but there is the expectation that it will hand things off; that it will not be in any particular business in the long term.

The long-term roles that we're talking about fall within two agencies. In terms of support of basic science, there's NSF, which has the responsibility of basic science for whatever cause under its banner, but NSF, too, because of its traditional modes of operating through the university community, through institutions which are really governed based on a sort of 3-year-grant cycle type of thing, again has the greatest difficulty of sustaining anything in the long term.

Now the long-term observations which are critical to—they're not adequate enough but they're critical to obtaining a global perspective on things. That role is NOAA, both in the atmosphere, the ocean, the ice, geophysical data itself, with some exceptions when you get to the land surface. The biology is not so obviously NOAA, where there are other agencies like USDA, and so forth, that come in, but no agency that has a global role on the land surface areas.

So what you're seeing here is that to put together the pieces we've got to exploit the traditional roles of those three agencies, because all of them are essential. And declaring a lead agency as a coordinating mechanism would probably be a disaster, given that split.

So what could work? Well, I don't pretend to have any great wisdom, but the least common denominator of solutions that I've heard of has sort of three levels to it. The first is a sort of program cross-cut function that could only be done by someone like OSTP, within the White House. It would require one or two additional staff in OSTP to do it, but it could probably be done—to keep an overview of what is happening across the agencies.

If you bring that down to a lower level like the present National Program Office, it would almost certainly work negatively. But at the highest level it could work.

Now that's just simply a cross-cut function, documenting what, so to speak, the plan is.

The second thing that's important to that, however, is some sort of Federal coordinating committee. A revised version of the present two fix-it committees in this area might be able to do the trick.

The third interface—that can worry about the details of implementation between the Federal agencies. The third interface you've got to worry very, very much about is the long-range inputs from the scientific community, and that could be done through a standing committee of the Academy reporting to OSTP, in practice reporting to members of the fix-it committee.

Now I say that because in the Global Atmospheric Research Program we did have examples of, if there is a clear address and a clear overall program goal, Academy committees can actually work in terms of getting inputs into the Federal mechanism.

Finally, the last and equally important mechanism is some sort of view in Congress that this is, indeed, the way we should be going and some sort of, let's say, encouragement from Congress to make sure that the Federal agencies appreciate the importance of keeping this together.

Now I'm not pretending that is an ideal answer. I don't think that is an ideal answer, but it's the best one that I've heard.

Dr. Botkin. I'd like to make a few comments.

The cooperation is really at three levels: The interagency cooperation within the U.S. Government, which Francis has spoken about very nicely, and then there is interdisciplinary cooperation among the scientists, and then there is international cooperation among nations to do this. I just want to say a few words about the second two.

The interdisciplinary one could be improved if there were specific language from Congress or from the Federal Government. For example, NSF operates in very disciplinary lines, and it's notorious among my colleagues that whenever they try to put in a proposal which is between disciplines, no panel will take it because it falls through all the cracks.

Similarly, if there were language to NASA that said there should be support for interdisciplinary programs, then there's the interest, and that interest could then be realized.

In terms of international cooperation, I believe again we see the beginnings of the programs such as I mentioned briefly and some specific language from Congress could be very helpful if it were to say that at least these were good ideas or to support them.

Thank you.

Dr. Suomi. Can I make a statement? Well, on this vexing problem, I would like to say something positive. And that is, thanks to private industry, we now have some fantastic tools for assembling and storing and making available data in both the recording media—the new laser disks and so on—as well as the computers with which to extract them.

So even though trying to digest much of this data is like trying to take a drink from three fire hydrants without drowning, the tools are now available so that we can do that.

And so if we followed some of these initiatives things would fit into place because of the need to have certain formats and standards, and so on. Otherwise, if we went off in our own direction, it might be a step backward. If we don't do it carefully, the important science will be lost in the records as well as being lost in nature, and so the records must be available.

Along these lines, I would like to say something else positive. NSF has been sponsoring a project called Unidata where the universities are getting together to get access to data, and it's just past the beginning but it looks very promising.

Thank you.

Mrs. Meyers. Thank you.

Mr. Nelson. Dr. Suomi, do you think that there should be a balance between operations and research on the meteorological satellites?

Dr. **Suomi**. Yes, I do, for the following reason: There are two elements in the observations from space which we need in meteorology and oceanography as well as some of these other things. One is called the initial conditions of the atmosphere or the initial state of the climate, and so on. Those are the operational ones which have to go on and on.

But in order to develop the instrumentation that will allow us to understand the processes which are responsible for changes in those boundary conditions, we need another kind of satellite which can observe processes. These do not have to go on forever, because once we learn how the process works and we can state it mathematically, one can go from there.

But, you see, the notion behind the operational one is for long lifetime, low cost, and this is the basic driver, rather than the research one, there the driver is to answer the question. And it might not necessarily mean a highly economical satellite, although in some instances it could be. So the sophisticated instruments which we need to get our first understanding I think are different than the instruments which might be used for operations later on, although the history, for example, of the Nimbus satellite, which was a test bed, made it possible to have a very economical operational satellite.

This was done for polar orbiters. We do not have that capability now for new stationary satellites, and perhaps this is something that should be pursued.

Mr. **Nelson**. Let me address this question to Dr. Bretherton and Dr. Botkin:

Do you think that the International Geosphere-Biosphere Program is an appropriate vehicle to coordinate international research efforts? Why don't we—either one of you.

Dr. **Bretherton**. Do you want to go first, Dan?

Dr. **Botkin**. Go ahead.

Dr. **Bretherton**. All right.

It's too early to say, in my view. As I see our problems of international coordination, there are three things that have to be done:

First of all, there has to be a forum in which scientists from the various nations of the world can effectively meet together to discuss what the scientific problems are, to discuss strategies for approaching those problems, to get some sense of priorities so that they can go talk to their own governments about things in some sort of coherent, unified way. That's a nongovernmental effort, of which the International Council of Scientific Unions, which is the, let's say, sponsor for the IGBP, is entirely appropriate.

The second thing that's needed is an intergovernmental—that means an international agency—an intergovernmental framework to deal with at some level routine but at other levels terribly important issues like data exchange, exchange of scientists, access, for example, by ship to someone's exclusive economic zone to go and take measurements for this program. And these are politically sensitive issues in many parts of the world.

And it's not obvious that data exchange takes place easily, and we need some sort of mechanism internationally that provides the framework within which these problems can be worked out.

The third type of international collaboration that's needed, particularly in the space area, is bilateral, which are fundamentally aimed at saving costs, at sharing the load of putting a space program of this sort, which is extremely expensive and where, in the nature of things, what one country does, if the data is properly available and properly exchanged, can be used for many different purposes and does not have to be duplicated everywhere.

Now those are three different functions.

It turns out that in the area of the physical climate system that is in place now, the world climate research program—now although that's not a perfect vehicle for what I'm talking about, it has other objectives besides simply looking at the changes over years to decades to centuries.

Nevertheless, I think it is an effective mechanism for probably all three of those. It turns out that it is able to work with—or has been able in the past to work with the various space agencies, and through the World Meteorological Organization, to get the degree of coordination that seems to be needed.

On the other hand, it does only cover half the problem. The other half of the problem is related to the chemistry and the biology and the land areas of the world. There we have nothing.

And the International Geosphere Biosphere Program is a first start at promoting the forum internationally to do this.

Mr. NELSON. Dr. Botkin.

Dr. BOTKIN. Yes, I'd like to add a few things to that.

I agree with Francis—it is too early. It's a little bit like parents expecting a child and discussing what medical specialty their offspring will go into—to ask if it's yet the right vehicle, because it's still a pregnant idea, I think.

It has the potential to fill this first need, this need for a forum. I think in getting the right kind of international cooperation you need the forum for discussion, and then you have to have actual research institutes and a planetary observatory for observing era, the sort of thing we're hoping we can develop in Venice, and you have to have systems for managing the data and sharing the data. And you need those three aspects of any program.

The IGBP could be part of the first, and there's enough precedence for it, and it might succeed, but for it to succeed it needs to obtain sufficient support from enough nations of the world. And it's not clear that at the national level, not just the scientific level, there's truly political support and financial support. It has to develop ways so that the research would actually be funded and so that there was clear agreement about what would actually be done, and there has to be the kind of balance that needs to be obtained.

Francis referred to briefly—that we have so few ways of agreeing on how to look at the Earth's solid surfaces. There are just so many ambiguities, but there's a lot of specific things that this committee could focus on, particularly the need for balance, the need for what the mechanisms would be to fund this, to get agreements among nations, and to work out what specific issues would be addressed.

Mr. NELSON. Thank you.

Dr. Bretherton, what kind of remote sensing system do we need for the long term?

Dr. **Bretherton**. What we're trying to do—I mean the critical problem is the long-term observations. On top of that, we need a set of more specialized missions aimed at particular processes or what you might call targets of opportunity.

I'll spend most of my time answering your question dealing with the long-term observations which I think I'd like to call an "Earth-observing system."

Now let me remind you that, properly treated, I think we have some of the basic pieces of that in place already. I'd like to extend what Vern said about the need to supplement the operational instruments by remarking that a lot of the things that we're talking about actually take place in the data processing, and a somewhat broader view at the processing end would make a lot of difference to attaining our long-term objectives at the same time as our short-term ones, for a very, very small increment in cost. And it would be crazy to duplicate that. That's the first point I want to make. So we're building on the existing operational things.

In the long run I think the space station platforms, part of the space station complex, really do provide a unique opportunity for part of the totality of what we're talking about. They provide the opportunity, first of all, to plan instruments and data systems from the ground up with a view to a continuous sequence of flying a research instrument or flying an instrument in a research mode perhaps, learning how it works, learning how to process the data, and then transferring it in situ into an operational mode without having to completely restructure the whole organization, the whole data system that surrounds it.

And, in other words, it provides a way of reestablishing that link between research and development that I think has been so successful a feature of our past developments in this area, but somewhat fragmented at the moment.

The second thing that the platforms provide is the fact that the individual instruments we need we have to integrate; we have to look at the same scene in several different wave lengths at the same time, several different viewing angles, and the reason is to reduce the ambiguity in the interpretation of measurements. It's part of the technology of the measurement process itself that you require all these looks.

It's extremely difficult to do with our present sort of fragmented views. You have one satellite here; another satellite there; another satellite there. Even getting the data, let alone putting it together to look at the same scene, is almost impossible.

Now as soon as you take that integrated view, you discover that on individual platforms the power and the weight goes up. And so we need to fly satellites that are basically more capable than our present generation of ones, and that's an opportunity that comes from the space station platforms, either as an integrated platform or as the modular technology was a derivative from the Space Station Program.

The third and probably most important reason of all is the fact is we are talking about here—about sophisticated, expensive, ongoing systems. You don't want to throw the whole thing away every time a lightbulb burns out, as currently happens on a geostationary satellites.

What we have to do and have the opportunity to do is to go back and refurbish things, to change parts that need changing, to bring back things so that we can calibrate them properly and keep up our long-range standards, and to go through and to change one instrument in place rather than having to scrap the whole satellite every time we want to change it then. Now that requires a scheduled set of maintenance, and on those polar platforms something like 2 years is appropriate.

Now why do I say polar and why do I say platforms? In looking at the whole of the Earth, polar orbits, Sun-synchronous polar orbits, are a unique opportunity. It doesn't satisfy all our requirements. It does not enable us to follow the time continuity that is so important, say, in hurricane spotting where we use the geostationary satellites, but it does enable us that twice daily look survey which really enables us to get a grip on what's happening globally, and that's why the operational satellites, with the exception of the geostationary, are all in Sun-synchronous polar orbit.

Why platforms? Basically because there is an awful lot of work to be done, and there's more to be done than can actually be accommodated properly on one platform.

But to reiterate, we are really not just talking about space station as being the manned part in equatorial orbit. We're talking about using all sorts of capability associated with the long-term presence of man in space to change the way we do business and getting these long-term observations.

Thank you.

Mr. **Nelson**. Dr. Botkin, international cooperation, do you think it would be improved if we had some kind of international research center?

Dr. **Botkin**. Well, I have to think that, because that's what I'm involved in. Yes, I think that scientists—from the point of view of scientists, scientists as people get involved in questions and they get intrigued by questions. And then they begin to cooperate. And the problems of working between nations among scientists will disappear over the issues. So if you have an international research center and there are certain questions posed that scientists get interested in, then the international problems go away at that level.

Now I also believe that an international research center can be important from both a governmental aspect or an international relations in that there are certain analogs where governments have agreed to cooperate in the following way:

With these large-scale environmental issues, if there is not an international research center, then each nation has to hire its own set of scientists. You've got acid rain, for example, where you have the producers of acid rain and the recipients of it—each hires its own scientists to answer the questions, and it's like having hired guns. Instead you have an international research center where everyone agrees this is a place we will focus our attention and we will accept the results, and then you've alleviated some international conflict.

That's just an example, but I believe in terms of international relations and intergovernment relations an international research center can play a vital role, too. So I'd say, yes, at both those levels, scientific and international government.

Mr. **Nelson**. Are we doing enough applications research for remote sensing?

Dr. **Botkin**. No. That's the short answer, but there's a lot more to it than that.

I think that there's—one of the problems we've had with applications in the United States is that there's a lot of confusion about what applications means, and then this gets into whether things are funded or not. So a semantic problem becomes a real problem in terms of funding.

For example, to some people developing an instrument is research and using it in science is application. For scientists, using the instrument in science is research and applying it, say, to finding out where Canada's economically useful forests are might be an application. To research management, manager, finding out those forests is research, not application.

Now, first of all, we have a problem in what is applications, and that has led to a lot of confusion in the past. And depending on the politics and fads of the times, either applications is in or out. And things get dumped when they were not really applications; they just happened to be called that. I think that's the first problem.

The second, I think that there are a lot of things that are to our Nation's interest to know about. I think that just as we have a U.S. Geological Survey and we have a Weather Service that tells us where the weather is—what the weather is—it's just as important to a nation to know its distribution of biological resources and mineral resources. And if that's an application, it's also a vital national interest. And so we should be fostering research and the development of techniques for those things as well.

There's a lot more I could say, but, anyway, that's a true statement.

Mr. **Nelson**. Earlier in your testimony you had mentioned, along with acid rain, the situation of famine in Africa.

Dr. **Botkin**. Yes.

Mr. **Nelson**. What's your particular interest in that?

Dr. **Botkin**. Well, first of all, we can talk about a scientific reason. Of course, everyone as a human being is concerned about it, just as a catastrophe.

But from the point of view of the environmental aspects on a global perspective, there are several things—we want to know what has caused the situation with the famine and there's been a drought. So then you want to know what has led to the drought.

Now there's a lot of fundamental science underlying that. For example, a lot of devegetation and deforestation has happened in various parts of the world, including those parts of Africa. And there's a suspicion that if you clear the land of vegetation, you promote further drought and further increases in the desertification. That's only a suspicion. What we need is to do the scientific research, so that we can understand this interaction between climate and vegetation, so we can see if we are spreading deserts.

There are other ways that the atmosphere and vegetation interact that can affect climate: The greenhouse effect gases not only can change the average temperature, but they might have effects that will change the dynamics of the atmosphere and change where rainfall is heavy and where it's light.

So is that an answer to your question?

Mr. Nelson. Well, hasn't Israel pretty well proven a part of that theory—that by taking what looks like desert and then coming back in and planting trees, which absorb moisture through their roots and emit it through their leaves, that you break the cycle of the desertification? Is that true?

Dr. Botkin. I think that there's a—there are things like that to suggest that it is true. It isn't clear what the extent of this event is, whether it is local scale, regional scale, or global scale.

There is also some evidence from the clearing that's going on in the Amazon that, as you clear the Amazon, there is less rain downwind from where there used to be forest. So there are some pieces of evidence that are suggestive, but there is nothing definitive. And it's not clear how extensive in scale this is.

Mr. Nelson. Well, what—well, let me just tell you: We've got the large format camera——

Dr. Botkin. Uh-huh.

Mr. Nelson [continuing]. And then we have this camera [indicating picture], which Pinkie Nelson—no relation, but now my new colleague on the December flight—is holding last year. Now what do we know on those two that helps us with regard to the famine in Africa that you talked about?

Dr. Botkin. I don't personally know that that information has been applied directly. It has a lot of attention being applied. In fact, it would be very valuable if we could, first of all, use those just to track what the limitations of desert and limitations of vegetation are now, and begin to compare it with older imagery. And that kind of real-time monitoring could be very useful.

Mr. Nelson. Yes, Dr. Soumi.

Dr. Suomi. Having worked in this area a little bit, it was a real surprise when we had our very first simple Earth radiation satellites. Even though the desert is hot as blazes through our own experience, the net radiation over the desert is negative. This means, then, that the desert cannot get hot from the Sun beating down on it. That may come as a surprise to lots of us, but the reason is that albedo of the desert is high and reflects much of the solar radiation, and the temperature of the desert is also high, the atmosphere above it dry, so we lose a lot of heat. So the net radiation on a 24-hour basis of both the long-wave and the short-wave radiation tends to be negative.

And what happens here was shown by Professor Chiney some time ago—that the air is sinking over the desert, and this causes the dryness.

Observations of these kinds can be interpreted much more readily in terms of the global picture rather than the local picture.

It is true that the planting of trees will change the local picture and, if it's big enough, maybe part of the regional picture. We just don't know yet. But, as I tried to say earlier, we do have tools to treat the problems on that scale.

Dr. Botkin. The large format camera used by astronauts could be very valuable in this and related topics.

For example, just by accident, some satellite imagery discovered fires that have been burning in Borneo for a very long time and were very widespread and were completely unknown. And they have since been found to have burned a very large area. It's that sort of rare event which turns out to be very important biologically here, which our astronauts can begin to look for.

Dr. **Bretherton**. I'd like to comment on both instruments. The large format camera is a superb mapping tool. We never got such high resolution, high precision images in space, and in terms of seeing features, interpreting what's going on specifically, it's superb.

A hand-held camera has also—we've got a library of such pictures now, and they're a rather different utility. They're very good for seeing processes that we hadn't realized were going on before, particularly because the intelligent human being who directed them often understood what was going on, realized it would be interesting, and directed the camera directly at that.

Now in terms of the desertification, the hand-held camera, the pictures I've seen are astonishing in reminding us of how much dust there is coming off the desert. The duststorms that we are seeing now are enormously more intense than I think any meteorologist expected to see 20 years ago. And they're just part of the phenomena that we've got to live with, and you see them again and again and again from those hand-held pictures in ways that you don't really see very plainly any other way.

And so we have this—the hand-held camera also has revealed to us new processes in the ocean that we've never seen before. There are, again, some superb reminders of some of the cloud processes that we cannot get from any other instrument.

And so what we see here is the hand-held camera is extremely useful for opening our eyes as to what to look for quantitatively. The large format camera is perhaps a very good quantitative tool, but that has to be preprogrammed and directed at the specific targets.

Dr. **Botkin**. Could I add a comment? Francis just reminded me. We have some fancy devices as in Landsat, et cetera, for looking at solid surfaces and looking at vegetation, and we see remarkable things with them, but we're not terribly good yet at verifying what they tell us.

Now we did some research with people who are now in the astronaut corps looking at vegetation from helicopters and from aircraft to try to verify what we were seeing from satellites. And it's remarkable how well you can do when you get a little bit above the ground.

There's a potential for astronauts using the large format camera working with biologists to help us in this verification problem, because it's such a marvelous tool and because you have somebody you can talk to: Does that look green or doesn't it look green? Right now we have to compare crude Landsat scenes with a very precise scene seen from a helicopter. So there's a potential which somebody I hope will think about.

Mr. **Nelson**. You make me think of testimony that we had when we had a field hearing at the University of Colorado in which one of the archeologists there was telling us about space technology but on an airplane over—I can't remember if it was El Salvador or Nicaragua. It was one of the Central American countries—in trying to determine where the gravesite or, in the alternative, where the village, where the ancient village was after they had discovered where the gravesite was. And they were able to determine this by this infrared photography, by seeing where the more dense vegetation was.

And I asked them, "Well, how in the world—I mean, by determining the road, how—what does that have to do with the dense vegetation when you're talking about an ancient road thousands of years ago?"

And they said, because of the more packed soil, because of the road, it held the moisture more and, therefore, the vegetation was greater. And by—through the remote sensing, were able to determine where that road, and then they could work their way from the gravesite they had found the bones back into where was the ancient village. That's fascinating.

Dr. **Botkin**. Yes. I think that's an excellent example of how you can use this kind of viewing for many things. And it extends into studying other aspects of biology and ecology as well as the anthropology. You get lots of clues about why things live where or what their habitats are in exactly the same way.

Mr. **Nelson**. That was in Costa Rica.

Dr. **Botkin**. Costa Rica?

Mr. **Nelson**. Well, gentlemen, this has been fascinating. If everybody would just sort of hold tight, the bells have rung, and I'm going to have to run over and vote. We will dismiss you all, and then we're going to have our second panel here shortly with Dr. Allen and Dr. Morrison.

Gentlemen, thank you for a most stimulating discussion.

[Recess taken.]

Mr. **Nelson**. Good afternoon. And we will resume.

We're delighted to have Dr. Allen and Dr. Morrison now as we look at planetary science.

So if you all would share with us, your written testimony will be a part of the record, and I would ask you to just share with us straight from your heart.

STATEMENT OF DR. LEW ALLEN, DIRECTOR, JET PROPULSION LABORATORY, PASADENA, CA

Dr. **Allen**. Thank you, Mr. Chairman.

I thought I'd start with a very brief review of the current program just to set the stage for some of the questions and comments that you may really be more interested in.

I have placed I think at your place a folder which has in it some pictures and all that I'd like to refer to as we go along.

Mr. **Nelson**. OK.

Dr. **Allen**. JPL is a part of NASA. It is a part of Cal Tech. The mission that we're assigned is to explore our solar system, and we've done that for the last 25 years, have visited most of the planets now, obtained a good deal of science information of great value, and I think also brought back some of the drama and beauty of the solar system in which we live in a way that has been an inspiration to young people and of great value, I think, to the education process and to the understanding of the public of science.

In your folder I have placed the well-known images of Jupiter and Saturn that occurred from the flybys of Voyager I and II, and there were many discoveries there that were quite important.

Voyager II is continuing onto Uranus, and its trajectory has been shaped very carefully by the gravity fields of both Jupiter and Saturn, and it will arrive at Uranus on January 24 of next year.

I've placed in your folder a current picture, one taken a few weeks ago, of Uranus from Voyager. That picture is already somewhat better than what can be obtained from Earth. It should be the third one on the list there, if they happen to be stacked in order.

Mr. **Nelson**. Yes.

Dr. **Allen**. In that same frame is also shown four of the larger moons of Uranus as they are now seen from the Voyager spacecraft. That planet is different than Jupiter and Saturn in many respects. It's tipped on its side, unlike any other planet.

The rings and the moons and the atmosphere of Uranus are bound to be different than any that we have seen before, and so this encounter is going to be a different one than Jupiter and Saturn but is bound to also be a very exciting encounter.

The spacecraft will get a gravity assist from Uranus that will take it on to Neptune. It will get there in about 1989. In a sense, that will complete these decades of exploring almost all of the planets in our solar system.

Now the discoveries that the Voyager spacecraft made at Jupiter led to the formation of a lot more detailed questions which are going to be answered by the mission that is planned to take off in May, which is Galileo. That will enter into orbit about Jupiter, with its first encounter very close to the surface of Io, one of the major moons of Jupiter. Galileo will also deploy a probe that will descend into the atmosphere until that probe is destroyed by the heat and pressure of the Jovian atmosphere.

The orbiter, then, is going to go into a very intricate pattern which will cause it to visit each of the major satellites of Jupiter several times over about a 20-month period, and each of those fly bys will come much closer than Voyager did and will bring back pictures of many times better resolution and quality than were obtained from Voyager as well as other data.

One of the most interesting of those moons is Io, and I've put a picture in your folder which is a fairly recent picture, although taken some years ago, but a recent reconstruction of that imagery in a way that's really quite startling, that shows the eruption of one of the major volcanoes on Io which is throwing material up some 300 kilometers above the surface of Io. That volcano throws out a ring that's some 1,300 kilometers in diameter. So it's a huge, sulfurous volcano.

Jupiter and its moons are really a small solar system, and the study that we have will add to our understanding of the planetary history.

Mr. **Nelson**. Dr. Allen, now this photograph [holding up photograph] was this actually taken by one of our spacecraft?

Dr. **Allen**. This was taken by Voyager. It is actually a composite of several pictures that has been done fairly recently, in which the high resolution qualities were obtained from some of the black and white returns of the videcons, and then the color information was added to it in a way that did not detract from that resolution.

So it really is a remarkable job done at Flagstaff by the team there and contains information that really we have not seen before, although the pictures were available for us. So it's an excellent image processing job.

The colors are essentially correct, and the image, although constructed from several photographs, is an accurate image.

Now we have a spacecraft that's under development to go back to Venus and to peer through the clouds with a high resolution imaging radar and measure some of the surface characteristics of Venus. That's important, of course, because ground radar and recent flights of Soviet spacecraft have revealed characteristics of that surface which have surface deformations which are somewhat similar to those on Earth caused by the movement of the great plates of the Earth. No other planet shows those kinds of movements, and it's regarded as very important to understand the Venusian surface in more detail.

We also have a spacecraft that is approved to return to Mars and make measurements of Mars with a lot of attention to the volatiles on Mars whose history has been quite a mystery. The picture that I have there shows some of the layered sedimentary terrain which is indicative of previous water flow on Mars which laid down layers of material and may still contain layers of ice in them. A better understanding of those kinds of materials is clearly fundamental to any decisions that may be made in the future about a manned mission to Mars.

Mr. NELSON. You might be interested in getting your staff to get Dr. Sagan's remarks to this committee yesterday, which he, normally not supporting a manned versus the unmanned missions, thinks that we ought to do a joint manned mission in the next century with the Soviets to Mars.

Dr. ALLEN. Yes; I'm familiar with his views. As you know, the Planetary Society and a fair amount of cooperation with Tom Paine's National Space Commission has focused a good deal on the possibility of that as a future goal for the United States, and it certainly is a very interesting prospect. I will get those remarks.

Now in March of next year Halley's Comet will return. There is a lot of Earth-based observation that will be made and a lot of international cooperation that will coordinate and correlate those activities, and there are five spacecraft that will intercept them—from the Soviet Union, from Japan, and from Europe.

Those data are going to extend greatly the observations that were made by the recent intercept of Giacobini-Zinner.

Even these spacecraft, more complete in their instrumentation, will fly by Halley's really at enormous speed because Halley's in its retrograde orbit and the spacecraft in their orbits have very high encounter velocities. So there will be a lot that they will not be able to measure in that very brief encounter.

One of the missions that is planned by NASA is a mission with a new spacecraft that would be called Mariner Mark II, which would rendezvous with the comet and remain with it throughout its orbit as it approaches the Sun. That is from the period of time when it is quiescent to the period of time that is shown in that artist's rendition, when it will be evolving material from its surface that will ultimately form the coma and the tail that is so spectacular.

That spacecraft is also planned to put a penetrator into the surface of the comet and thereby determine better the characteristics of the pristine material of the comet.

That's, of course, of particular interest because comets are thought to be the residue of the very early processes of the aggregation of the fundamental material of which the solar system was formed, preserved by being so far out in the deep freeze of space that the characteristics have really not been changed over the 4.5 billion years since it was formed. So comets are very high in the science priorities of things that we need to do.

Now we've done a lot in the past 25 years. There's a lot more that we need to do that is not firmly planned. We need to go to Titan, the moon of Saturn, and measure the characteristics of that because it has an atmosphere that seems similar to what the Earth's must have been in its early stages.

We need to get samples back from comets, asteroids, and of course from Mars. We've learned from all of the things we've done so far how to look at the Earth as a planet, and some of the observations that we now know how to make that permit us to view the Earth nearly as a whole, nearly at one time, are fundamentally different than measurements that we've been able to make in the past and have been reviewed by some of the people who appeared here before you previously.

One of those of particular interest to us is the technique of measuring the height of the ocean from a satellite, and the graph that you have there is from an earlier spacecraft flight with a radar altimeter which measures the surface of the ocean with such precision that one sees there the indications of the surface features of the bottom of the ocean; that is, the gravity characteristics of the surface of the ocean are determined by the features on the bottom.

However, the experiment that we will make in Topex is really to measure the variability of that and from that determine currents. Those currents, of course, are of great interest in determining ocean circulation and will be combined with many other experiments in a world process of making ocean circulation experiments, and is of great practical interest. If one could have made those kinds of measurements at the time the great El Niño phenomena of 1982 and 1983 was occurring, then one would have been able to hopefully, at least with the benefit of hindsight, have predicted phenomena that had really great human effects on a major part of the globe.

We recently flew a telescope that I know you're aware of that surveyed the sky in the infrared and made wonderful discoveries. I have included in the folder a map which contains some 250,000 point sources emitting in the infrared.

The bright line through the middle of that is the Milky Way. That area of brightness is normally obscured in pictures one sees in the visible of the Milky Way because of the intervening dust. In this case one sees that dust itself and sees through it some of the places near the center of the galaxy where star formation is believed to be taking place.

The survey showed some other things that are of particular interest in the planetary field. It showed that there were a number of stars that appeared to have an excess of infrared radiation from what was expected by their color temperature, as measured in the visible. And that could only be interpreted in terms of saying that there must be an amount of cold material, dust probably, in the vicinity of those stars.

One of those stars was then subsequently observed by an astronomer from JPL and from the University of Arizona with some very special techniques that allowed them to block out the bright light from the stars and look for dim material near the star, and the results of their efforts are shown in the next picture that should be in your folder there.

Mr. Nelson. This was one of your scientists, wasn't it?

Dr. Allen. Yes. James Terrile of JPL and Bradford Smith of the University of Arizona made that from the great telescope in Chile. It's a picture of Beta Pictoris using a very special coronagraphic technique and a number of other astronomical techniques which stress the central image and the scattered light from that in order to be able to see that disk of what is relatively dim material. I say relatively. It is 10^8 less bright than the star. So you can see the great care which had to be taken in order to be able to make that particular picture.

Mr. Nelson. And with this picture it suggests that there may well be a solar system there?

Dr. Allen. Well, it certainly is a solar system in some sense. It is a material around the star which has already aggregated itself into a disk, and the disk has dimensions and characteristics which are not unlike those that are associated with the Earth when one includes what one believes is on beyond the orbit of Pluto.

Therefore, it is certainly a protoplanetary system at least.

Mr. Nelson. Yes. Is it in the same plane as our solar system?

Dr. Allen. I don't know the answer to that, but it would be surprising if it were. There's no reason why it should be.

Mr. Nelson. I mean, it doesn't have to be this way——

Dr. Allen. No.

Mr. Nelson [continuing]. If our plane of our solar system is that way. It may well in fact be that way?

Dr. Allen. Yes.

As a matter of fact, that imagery does not foreclose the possibility that there are planets already associated with that star. The star is considerably younger than our Sun, and we're unsure of the history of evolution in that period of time. The disk of material which one sees is really small particles, but that disk does not go all the way into the star. And although we wouldn't see them if they were there in this kind of pictures, there may, indeed, be planets that are associated with this particular image.

In any event, for the first time now we have directly imaged some sort of solar system around another star. The prospects that that opens, of course, are really just fascinating.

These techniques can be extended. There is this technique and there are other techniques as well which in the future will surely give us much more information about dim objects near other stars. That information, in my opinion, is almost certainly going to lead us to the detection and at least some measurements of the properties of planets about other stars, a prospect which really is an extremely exciting one.

Mr. **Nelson**. Is this a technique that can only be used on the ground or is it a technique that we can use in the Hubble telescope?

Dr. **Allen**. No. It can be used much better in space. The Hubble telescope, unfortunately for this particular technique, was fabricated for other purposes and, therefore, it is not convenient or effective to use this coronagraphic technique in the Hubble space telescope.

We have looked and are continuing to look at possibilities of modification to it further downstream; that is, the Hubble telescope will have instruments replaced in it as time goes on. But at the moment it looks less likely that that will be as successful as flying a special purpose telescope just for this purpose, which would be a much simpler instrument but specialized to this, which we would fly either on the shuttle or perhaps associated with the space station.

Mr. **Nelson**. I see.

Dr. **Allen**. So there are certainly ways, but they're highly specialized techniques that will permit that to be explored.

Mr. **Nelson**. I see.

Dr. **Allen**. Well, I only want to close by observing that, as you know very well, that we're going to enter a year of space science. It will start with the Voyager encounter of Uranus in January, Halley's comet in March, the launch of Galileo to Jupiter and Ulysses to the poles of the Sun in May, the launch of the Hubble space telescope, the flyby of an asteroid by Galileo en route to Jupiter—a whole array of science spectaculars. There's no question that we're in for learning a great deal more about our solar system, other stars, and maybe planets, and obtaining information which is going to be very valuable to not only science, but to those of us who live on this planet.

[The prepared statement of Dr. Allen follows:]

STATEMENT OF

DR. LEW ALLEN

DIRECTOR

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CALIFORNIA INSTITUTE OF TECHNOLOGY

before the

UNITED STATES HOUSE OF REPRESENTATIVES

SUBCOMMITTEE ON SPACE SCIENCE AND APPLICATIONS

COMMITTEE ON SCIENCE AND TECHNOLOGY

October 9, 1985

Mr. Chairman and Distinguished Members of the Subcommittee:

It is with great pleasure that I make some observations on the past, present, and future of the solar system exploration program and its attendant technologies.

The United States began exploring our cosmic neighborhood just about a quarter of a century ago. Our scientific automated spacecraft have visited all the planets known to antiquity and more than a dozen moons. We landed on some, probed the atmospheres of others and returned dramatic pictures of all of them. They became real places, no longer just points of light in the night sky -- places with wind and storms, craters and canyons, quakes and erosion, ice and sleet, lightning and volcanoes.

Mariner, Viking and Pioneer spacecraft investigated Mercury, Mars and Venus as the planetary program reached full stride in the 1970's. While we were gaining a much better understanding of the close-to-home terrestrial planets, Pioneers 10 and 11 ventured across the Asteroid Belt to sample the violent Jovian environment, leading the way for the Voyagers.

Arriving at Jupiter 4 months apart in 1979, Voyagers 1 and 2 began the most spectacular episode in the brief history of solar system exploration. Auroras and superbolts of lightning were found crackling through the Jovian atmosphere. A tenuous ring of particles circling Jupiter was a totally unexpected discovery. Nine erupting volcanoes were detected on Jupiter's moon Io, the most geologically active body now known in the solar system. Volcanic plumes extended some 300 kilometers above the surface, and material was being ejected at velocities up to 20 times that measured at Earth's most explosive volcanoes. One of the nine had shut down prior to Voyager 2's arrival. Three new satellites were discovered.

Before the first Voyager observations of Saturn in 1980, the planet was thought to have 11 satellites. Today it is known to have at least 20 -- three found by Voyager 1, three by Voyager 2 in 1981 and three more in recent ground-based observations. The Voyagers also detected winds at Saturn's equator blowing at more than 1000 miles per hour and showed that the spectacular rings number in the hundreds or perhaps thousands -- most of them concentric, several eccentric and a few twisted, or braided; and that the satellite Titan's atmosphere is twice as thick as Earth's and is composed primarily of nitrogen and may produce methane rain, rivers and glaciers.

The Voyagers, as you know, are still on line -- as it heads toward interstellar space, Voyager 1 is searching for that remote boundary zone where our sun's influence gives way to the other forces of the galaxy.

Voyager 2 is less than four months from its close encounter with Uranus. In fact, in just a few weeks, the spacecraft will begin the systematic far encounter observation of the planet. If we're lucky, as well as intelligent, Voyager 2 will continue operating long enough to reach Neptune in 1989. These planets are so distant that Earth-based observations have not been very informative. We hope that Voyager will help us understand the chemistry and motion of their atmospheres, and give a better understanding of the apparent differences between them and the nearer gas giants, Jupiter and Saturn.

Twenty-five years of solar system exploration have yielded a number of benefits. The first of course is scientific discovery. The basic objective of the program has been to gain a much better understanding of the solar system, of the planets and their evolutionary history. We have met that objective. Further, we have answered some questions (and posed many new ones) on the origins of life and the existence of life on other planets. We have derived from these observations a significantly better understanding of how to study the Earth as a planet and how to take advantage of opportunities for wide-scale and closely timed observations of our planet.

In addition to the scientific knowledge obtained, a secondary benefit to society comes from the educational aspects of space exploration and from the public interest shown in it both at home and abroad. The plaentary missions have resulted in widespread dissemination of imagenary and data which has inspired and broadened the view of people around the world. School children are excited by our new discoveries and motivated to learn more. U.S. prestige has been enhanced by the leading edge programs which have characterized our solar system exploration activities. Another point is the significance of the program's technology benefit. Some of the examples of the autonomous operations of spacecraft under conditions of communication delay and stressful environmental conditions have been beneficial to other programs. Certainly the Deep Space Network experience in working with very low signal strengths and other communciations problems peculiar to planetary missions has contributed to a number of technologies benefiting other programs.

Further, I think that some of the capabilities that have been developed in these programs over the last 10 years have become beneficial to other agencies of the government. In recent years we at JPL have applied some of our skills and capabilities to DOE alternative energy programs. We are now applying them to DOD programs, to actitivies for the FAA, and are putting forward some significant efforts in attempting to find commercial value from the results of some of these programs. It is my own view that when one weighs the totality of these benefits, the cost benefit ratio of planetary programs is quite good. It has resulted in resounding scientific successes, a lot of public interest, a great deal of educational value, and important technological advances.

Our next step beyond Voyager in piecing together the puzzle of the outer planets is clear -- a graduation from initial reconnaissance flybys to atmospheric entry and long term observation from orbit. In a word, Galileo.

The Galileo mission, to be launched next May by a Space Shuttle and Centaur upper stage, will be the first to penetrate the atmosphere of Jupiter, sending an instrumented probe deep into the cloud layers. It will be the first mission also to orbit within the Jovian system, spending nearly two years there making multiple close passes by the four major satellites. During a one-hour descent through the atmosphere near Jupiter's equator, the entry probe will gather, and relay to Earth via the orbiter, direct measurements impossible to obtain from overhead. The atmosphere may hold clues to the nature of the original "star stuff" from which all planets formed and to the dynamic meteorological changes observed in the swirling cloud layers by Voyager. The intense heat and crushing pressure will determine the duration of the probe's brief mission. Then for at least 20 months, the orbiter will fly an ever-changing path about Jupiter, coming 100 times closer than the Voyager flybys of Europa, Ganymede and Callisto and 20 times closer to volcanic Io. We can well imagine the new information possible in the Galileo pictures and other measurements. Throughout the orbital mission, instruments will monitor surface changes in Jupiter's atmosphere, and others will measure the tremendous Jovian magnetosphere, more than 100 times the volume of the sun.

NASA's has identified the next calendar year as the "Year of Space Science." And rightly so. During 1986 there will be a sequence of space activities of unprecedented scope which promise a major surge of knowledge. Chronologically, these events are as follows:

- o In January, Voyager 2 reaches Uranus to give us our first close look at the planet that appears to lie on its side as it orbits the sun some two billion miles away.

- An international armada of five spacecraft will cross paths with Comet Halley in February and March, taking advantage of the once-a-century opportunity to investigate the famed celestial visitor.

- During a single week in May, shuttle orbiters Challenger and Atlantis will initiate the long, complex voyages of Ulysses and Galileo. Each will be propelled from Earth orbit onto a Jupiter transfer trajectory by the newly developed, Shuttle-compatible high energy Centaur upper stage.

  - Ulysses will use the gravity of Jupiter to leave the ecliptic plane for a swing back through the inner solar system to the polar regions of the sun.

  - Galileo, the intricate combined Jupiter orbiter and atmospheric probe, described here earlier, is the showcase mission of the U.S. planetary program.

- The long-awaited Space Telescope -- the astronomical/astrophysical observatory in the sky -- is scheduled for a Space Shuttle launch in August. The new telescope will peer far into space and back in time, producing images of unprecedented clarity of galaxies and star systems.

- Finally, year's end brings the likelihood of the first close encounter with an asteroid. The Jupiter-bound Galileo spacecraft is planned to be launched on a trajectory that allows a December flyby of an asteroid at a distance of several thousand miles.

Even as Galileo flies the long course to Jupiter, another U.S. spacecraft the Venus Radar Mapper, will be launched (April 1988) on a three-month trajectory to Venus. By mid-1989, the mission will provide a map of the planet using radar to penetrate the thick cloud cover. A synthetic aperture radar (SAR) system, similar to imaging systems flown on the Space Shuttle (SIR-A and SIR-B) for Earth observation, will be VRM's single instrument. The SAR takes advantage of spacecraft motion to create a synthetic aperture corresponding to an antenna many times the real length. The VRM instrument, in addition to its primary function of imaging, will also provide altimetry and radiometry. From these observations we hope to understand some of the geological processes that form and modify the surface of Venus, a planet quite similar to the Earth in size and mass. Does it, like the Earth, have plate tectonic activity? Perhaps we will find evidence of ancient rivers and oceans on a surface that is now much too hot for either.

Following VRM's renewal of terrestrial planetary investigation, Mars will be revisited by the first of a class of low-cost spacecraft, the Observer series. The Mars Observer will orbit the planet, studying aspects of the Martian climate, atmosphere, geology and geophysics and will examine the relationship between the surface and atmosphere over a full Martian year (687 Earth days). The mission will address questions raised by previous Mariner and Viking missions. We wish a better understanding of the structure and circulation of the atmosphere and the effects of seasonal change. The Viking lander revealed a very complex surface chemistry on Mars which we wish to explore more widely and in more detail.

Over the last year, observations by the remarkably successful Infrared Astronomical Satellite (IRAS) have led the IRAS investigator teams to conclude that several nearby stars are surrounded by clouds of cold, solid material. Prompted by this discovery and enabled by recent advances in charge-coupled device (CCD) detectors, optical design of ground-based coronographs and image processing techniques, a disk of orbiting circumstellar material around the nearby star Beta Pictoris has been directly imaged. The material takes the form of a highly flattened disk rather than a spherical shell, and this indicates almost certain association with planet formation. Disk brightness distribution analysis indicates that the central region of the disk, comparable in size to our own solar system, has been swept clear of material. The only complete explanation for this observed void is the removal of the larger particles through the planetary accretion process. Although we have not yet directly observed planets in this distant solar system, the indirect evidence for their existence is persuasive.

The disk of matter around Beta Pictoris, 50-light years away, may very well be a direct analogy of our solar system Oort cloud thought to be the source of comets. By looking 50-light years away, we may be viewing a form of the distant past of our own solar system. We also can look within our solar system to ascertain facts from our distant past.

Comets and asteroids, and their relatives the meteorites may very well be our best window on the origin of our solar system. Large objects such as the planets have modified themselves chemically and physically over the 4.5 billion years of solar system history. The comets and asteroids are small enough so that many of them may have retained their primitive characteristics.

A series of missions to these primitive bodies has been initiated by the recent investigation of Giacobini-Zinner with the ICE spacecraft. Exciting and puzzling results came from this initial encounter. Further comet data will be supplied by the visit to Halley in the next year by the Europeans, Japanese and the Russians. The United States hopes to provide the definitive comet mission with a flight to a short-period comet in the early 1990's to rendezvous with its nucleus and actually send a harpoon into it, thereby obtaining direct spectroscopic analysis of its composition. This mission, Comet Rendezvous and Asteroid Flyby (CRAF), is the first of the SSEC core missions to use the Mariner Mark II spacecraft series. With CRAF we hope to determine not only the physical nature of a comet but also its chemical make-up; the minerals and ices on and below the surface of its nucleus and in the cloud-like coma that surrounds it. We believe will be sampling material left over from the origin of the solar system.

I can imagine that more planetary science might be done in the near future. For example, there might occur really dramatic new science discoveries that would catch the imagination and inspire people to undertake substantially larger efforts. I personally think that the existence of opportunities for the examination of other planetary systems is a justification for an expanded program. I hope, as we better define the kinds of observational techniques which will yield additional information on these other galactic planetary systems, that the cost of doing these special activities will not be subtracted from the present level of effort. I think it represents a truly dramatic and exciting motivation for further work, and for the expansion of the planetary program funding level. I have every expectation that before the close of the 20th century this exciting work will result in some very fundamental discoveries.

Beyond the 15 or 20 years planning period with fairly clearly defined missions and objectives, where might we be? What sort of things are we trying to address? I believe, that NASA will remain a civilian organization, devoted to exploration and not to applications; an organization increasingly influenced by the desire to conduct programs in an international environment, with increasingly intimate cooperation. Even though there are some signs at the moment of factors inhibiting that kind of cooperation in some areas, e.g., the concerns about technology transfer, it's my own prediction that those will not, in the long run, be seriously inhibiting. The degree to which the U.S. and the Soviet Union cooperate in civilian space exploration endeavors is, of course, a more difficult one to predict. And I'm not going to attempt that prediction in any very significant way. But I do think international cooperation is likely to prove useful and important, and therefore is likely to be an increasing part of the NASA activities. In that future time period, there's no question that space activities will be characterized by both manned and autonomous missions.

I'd like to describe what seems to me could be an integrated planetary science program for the future. The first of these is, as I mentioned, the opportunity for continuing exploration, to detect and examine other planetary systems. The IRAS telescope revealed a number of stars with infrared excesses. And the observation of Beta Pictoris by Drs. Richard Terrile and

Brad Smith shows the characteristics of what is at least a protoplanetary system. It is very exciting and these discoveries lead to the expectation that more refined observations can be made, more systems can be examined. We have an opportunity to get major new insights into solar system evolution by observing other planetary systems at various stages in their evolution.

With the kinds of instruments and observational techniques that one can easily imagine being available in the time period that we're addressing one ought to be able to observe those planetary systems in sufficient detail to identify clearly the existence of planets. One can presume they do exist. One can also make sufficient observations to determine the status of their evolution and whether they are conducive to the formation of life. That would be very exciting, I think, and is worth a good deal of consideration.

There will certainly be, at the same time a remarkable degree of progress in the ability to understand the Earth as a whole...the Earth as a planet. Satellite observation is a relatively mature area with increasingly capable techniques for making global and synoptic measurement of oceanic and atmospheric behavior. Such data will be correlated with very sophisticated numerical modeling in a way that will give one the ability to understand the climate, the ecological circumstances of the Earth, and man's influence on the atmosphere. The results will be fundamentally different from our present ability to understand and observe.

JPL, which has had an active Earth-sciences program for many years, will conduct several projects to study Earth as a system of interrelated, interacting parts, where the troposphere, stratosphere, oceans, ice, solid earth, and land surfaces function as a single, organic whole.

Two of these -- the Ocean Topography Experiment (TOPEX) and the NASA Scatterometer (NSCAT) -- will provide further data on air-sea interactions; TOPEX, a free flyer, and NSCAT, which is to fly on a Navy research satellite will study ocean current and winds, respectively. This information is critical to understanding specific phenomena, such as the El Nino ocean-warming cycle of 1982 and 1983, as well as more general trends, such as the role of the oceans in the formation of weather and climate.

Other JPL efforts, such as the Atmospheric Trace Molecule Spectroscopy experiment and the Microwave Limb Sounder, will gather data on the atmosphere. Further in the future, JPL experiments may fly on the Earth Observing System platforms, companions to NASA's Space Station, to continue the work of these earlier missions.

Instrument development, data analysis and archiving, laboratory research, and theoretical studies will continue at JPL as part of the prospective international effort to improve our understanding of the Earth system.

Exploration of the solar system, will, during that period, extend to regions beyond Neptune. I think the ability to observe primitive materials beyond Neptune will evolve for us, and detailed study of critical issues on the nearer planets will be conducted.

The core planetary program defined by the Solar System Exploration Committee (SSEC) provides only limited stimulation for new technology. We need to consider other ways to develop technologies with value to the program and with significant value in other areas -- perhaps to the military or to gound-based programs.

One of these technologies is large reflectors in the optical, IR, and millimeter wave regimes. With the techniques now available in segmented optics, such as the 10-meter Keck telescope, or the multiple mirror telescopes now pretty well in hand, it seems to me that there's really very little limit on the size of the optical instrument that one can build. There certainly may be some cost limits, but to go to a 10-meter telescope in space is a program I think we could define now and carry out with low risk. Life gets very exciting when one gets up to 60 meters. It is not beyond question in that time period to consider 60-meter optical instrument, operating at the diffraction limit in the optical regime. And it is certainly not beyond our capability to think of that as a moderate risk activity in IR and millimeter waves. If one moved in those directions, clearly there are things one can do that would be very interesting.

An Earth observing instrument now under development will require substantial improvements in the technology of data processing. This is the imaging spectrometer which uses CCD arrays with one dimension spatial and the other spectral. These devices are very powerful for certain remote sensing applications, allowing one to identify plant species, for example, with a considerable degree of precision. But such devices suffer from very obvious limitations. The first is that the system can be flooded with data very quickly, and one must figure out how to handle such a data volume. If you pipe it all down, you get into downlink difficulties fairly quickly. If you try to process it on-board, a typical concern is that no algorithm available today will identify the target species in the presence of a mixture of species.

But the future of this potential seems to be almost unlimited. The way that computational power is increasing and the way it can be forced to increase further, will lead to theoretically unlimited capabilities for handling data at those kinds of rates and complexity.

One of our near term goals is to design the spacecraft and the ground system in such a way that they become transparent to the user. Then scientists and investigators who participate in the spacecraft mission could operate with their instruments as though there were no intervening systems. They could operate an instrument as though it were in their laboratory and under their immediate control. That again, is a capability which one can talk about now, but which is really quite a way from being implemented. Yet this problem should yield to the techniques of computation and continuing development of intelligent systems.

Today's program is no less intriguing than the possibilities of the future. I hope that some of you will be able to join us in Pasadena next January for the Voyager 2 encounter with the planet Uranus. That event will be a first in planetary exploration as well as another first in a long string for the United States. But Voyager is an elderly spacecraft developed more than a decade ago and operating well beyond any warranty period.

Other space-faring nations are challenging our pre-eminent position in solar system exploration. They may overtake us. That's yet to be seen. That thought is alarming, at very least to our intellectual sensibilities. The Galileo Project, designed to conduct a truly comprehensive investigation of the Jovian system, takes the edge off that negative possibility in the immediate near term with Galileo launch scheduled for next May. And with a firm commitment to the level-of-effort core program recommended by the Solar System Exploration Committee, plus the flexibility to pursue appropriate intriguing space science opportunities and their enabling technologies, it is highly likely that America will remain in the forefront of plaentary exploration.

Mr. **Nelson**. Thank you, Dr. Allen.

Dr. Morrison.

STATEMENT OF DR. DAVID MORRISON, LUNAR AND PLANETARY LABORATORY, UNIVERSITY OF ARIZONA, TUCSON, AZ

Dr. **Morrison**. Thank you, Mr. Chairman.

I certainly share Dr. Allen's enthusiasm about the prospects we have for the near future in planetary science and in space science generally. It will be a very welcome year, coming as it does after a period of 8 years in which there has not been a single planetary launch of spacecraft, to continue the kind of intensive exploration of the solar system that we became sort of used to during the previous two decades.

But we are in a much stronger position now, and I also am very pleased at some of the trends that I've seen even displayed in these 2 days in Washington—the great commonality of interests between planetary scientists and the rest of the astronomy-astrophysics community in the question of the formation of other planets and the detection of planets around other stars; the common interest between planetary science and the Earth science community where we think now of the Earth as a planet and, indeed, the other planets as worlds, too, in which we can all work together and use the same data and many of the same techniques to approach common problems.

I could talk enthusiastically, I'm sure, about some of the science we've done and some of the prospects for the future. There is a limitation on time here, and perhaps I should venture into an area in which I am less expert but also feel rather strongly, and that is that some of us have worked very hard in the planetary community in recent years not only to contribute to our understanding of the solar system, but perhaps to make some small contribution toward how to carry out planetary missions in a more economical and effective way.

I am here as the Chair of the NASA Solar System Exploration Committee, which was a committee appointed to make recommendations concerning planetary missions from the present through the year 2000, but to do so in the specific context of limited overall national budgets for space science, to see if there's a way we could revitalize what was at that time an extraordinarily sick planetary program, and to do so without having to achieve the spending levels that we had had in the previous two decades.

The Solar System Exploration Committee, as you well know, made a series of recommendations. The first part of our recommendations concerned a core program of planetary missions which was intended to be a minimum level program, something that could sustain our capabilities, could maintain the interest of the scientific community, could let us train new planetary scientists, and could, indeed, continue a flow of exciting new data from the exploration of the planets, but do so at a minimum budget level.

And an essential part of that plan was a careful timing of missions so that we could make the optimum use of human and material resources, so that we could have the maximum inheritance from one mission to another.

We're very pleased that that program has been received well by NASA, by the Office of—the OSTP, by your committee and other committees of the Congress, and, indeed, we would like to think that we have shown the kind of discipline that has led to success in the last few years; that we are dealing now with a level-funded planetary program at something less than half the funding level that was associated in the early seventies, for instance, when we were developing Viking and Voyager, but has, nevertheless, allowed the initiation of the new missions to Venus, to Mars, the continuation of Galileo, the things that Dr. Allen has talked about.

I raise this issue with you partly because I was asked to speak to the future and partly out of concern because a recent issue of Aviation Week has reported that for the first time in 3 years it may be that the administration budget request will deviate from the SSEC recommendations in a very essential way; namely, the development in fiscal 1987 of the Mariner Mark II spacecraft which is to be the workhorse spacecraft for planetary exploration through the nineties and which was timed to take optimum advantage of the expertise, the engineers, the technicians, the managers at Jet Propulsion Laboratory primarily who were coming off the Galileo Program in that timeframe and who can be—can transfer their abilities and their knowledge in the optimum way into constructing this new spacecraft.

Obviously, this is not a budget hearing and the Federal—the administration's budget proposals will not be out for many months, but I want to tell you that we in the scientific community are very concerned about this because we don't want to go back into the old way of doing business with individual missions fought for as individuals, developed and operated as individuals in a fairly expensive way; then long intervals in between with nothing, and then sort of start over and bootstrap your way up again from one mission to another.

We think it is important for the cost-effective use of the taxpayers' money as well as for our own optimization of the science and exploration of the solar system that we do try to pursue a well-planned policy that makes the best possible use of limited resources.

Specifically looking to the future, we hope that this core program, as recommended by the SSEC, will be continued; that it will involve missions in the near future—the comet rendezvous mission that Dr. Allen referred to; following that the so-called Cassini mission, hopefully carried out jointly with the Europeans, to Saturn and Titan; and a variety of other missions as we get into the late nineties.

The Solar System Exploration Committee is also just now in the process of completing a report which I'm sure will be available to you in the spring which goes beyond this minimum core program and defines the missions we feel would be the most productive if we could, indeed, address more technologically challenging problems and have more funds available to do so.

And as I think you know, our first priority there is a mission to Mars involving a rover on the surface and the—as a part of the process of exploring a wide range of terrains on the surface, the selective picking up of samples and bringing them back to a central point for return for analysis in laboratories on earth.

This is just one step in the direction of increasing our planetary involvement, really at a level no greater than Viking which was done in the midseventies, which could, in turn, lead, we hope, to a greater human presence as well as scientific presence in the solar system, and I think it's—I can speak for myself at least, and probably for most members of the planetary community, in saying that the ultimate goal of intensive exploration of Mars and even of human exploration on Mars is one that is tremendously exciting to us and is, I believe, a direction in which our science and we as humans will wish to move.

Thank you.

[The prepared statement of Dr. Morrison follows:]

PREPARED STATEMENT OF DR. DAVID MORRISON

Chairman Nelson and Members of the Subcommittee: thank you for this invitation to discuss the recommendations of the NASA Solar System Exploration Committee (SSEC) concerning planetary exploration missions through the year 2000. In this testimony I will describe the SSEC and its charge; summarize the recommendations of the SSEC for a minimum, Core Program of planetary missions; describe the more recent Committee recommendations concerning a possible augmented planetary exploration program; and comment on the current status of these recommendations and of the NASA planetary program.

The Solar System Exploration Committee of the NASA Advisory Council was formed in 1980 at a time of crisis in the planetary program. After two decades of spectacular successes, the NASA planetary program had nearly come to a halt, and many persons felt that planetary missions were too expensive for the limited space science resources of the 1980s. Indeed, there was widespread talk of the U.S. dropping out of planetary exploration, leaving this aspect of space exploration to the USSR and the European Space Agency. Against this background, the SSEC concentrated its efforts on identifying a more efficient and less expensive way of carrying out the exploration of the solar system.

The initial recommendations of the Committee, presented in 1983, defined a minimum-level, cost-effective, Core Program of planetary exploration missions. The Committee wrote then that it "recommends that the United States planetary exploration program be based on a Core Program of missions that meet the following criteria: high scientific priority; moderate technological challenge; and modest cost. By addressing highly focused scientific issues these missions, though restrained in scope, are capable of making substantial progress during the next two decades toward the goals previously identified by the National Academy of Sciences' Space Science Board. The Core Program will provide a stable base within which NASA and the universities can maintain the capability for planetary exploration. The program will draw significantly on the capabilities of the aerospace industry and will provide the basis for subsequent augmentation with more challenging missions."

The basis upon which the missions of the Core Program were selected was the science strategies developed by the National Academy of Sciences' Space Science Board. These strategies were brought up to date by four working committees of planetary science specialists including members of the Space Science Board's Committee on Planetary and Lunar Exploration.

Throughout the previous twenty years of planetary exploration the Program pushed technology in many areas: launch capability; telecommunications; spacecraft reliability and automation; and atmospheric entry techniques. Meeting these technological challenges has been expensive, but it also has endowed us with a strong technological base that can be exploited to support relatively low cost missions of high scientific priority. These are the missions of the Core Program. To achieve the high inheritance required to keep costs down, the missions will use: available spare hardware; derivatives of industry-built Earth-orbital spacecraft; a new, simple deep-space spacecraft of reconfigurable design (the Mariner Mark II); and an updated, multi-mission operations system.

The missions of the Core Program were selected and timed so as to fit within a level-funded planetary program, supported at less than $400 million per year (FY 86 dollars). By insisting upon strict discipline and maximum inhertance, aimed at making optimum use of human and material resources, we believed that an exciting exploration program could be sustained at only about half the cost that had characterized the previous decade.

The first mission of the SSEC Core Program was the Venus Radar Mapper, approved as a FY 84 new start and now scheduled for launch to Venus in 1988. Subsequently, there were two main elements of the Core Program.

First were a series of Planetary Observers, a new class of low cost, modestly scaled, inner solar system missions using already developed, high capability Earth-orbital spacecraft. The Mars Observer, the first in this series, was approved for a FY 85 new start and is progressing well, and plans are under development for other Observer spacecraft to study the Moon and Earth-approaching asteroids.

The second and most important new element of the Core Program were the Mariner Mark II missions, based on a new, high-capability, modular spacecraft being developed at the Jet Propulsion Laboratory. The Mariner Mark II will be the workhorse of planetary exploration in the 1990s. The first of these missions is the Comet Rendezvous/Asteroid Flyby (CRAF) mission (FY 87 new start), and the second is a joint US-ESA mission to Saturn and Titan called Cassini.

With level-funding for the NASA Solar System Exploration Division and a high degree of discipline and management skill, the

SSEC anticipates that it will be possible to launch approximately four Mariner Mark II and four Observer missions during the decade of the 1990s.

The next step beyond the Core Program will require an increase in funding for planetary exploration above current levels. Only if the nation can afford such an increase will it be possible to undertake the more ambitious and technologically challenging missions of the SSEC Augmented Program. However, to place these larger missions in proper context, I note that their cost and complexity are in the Viking class, very definitely not in the Apollo class. These are missions that we believe the U.S. can and should support, just as it supported Viking and Voyager in the 1970s.

The SSEC has limited its recommendations to just two missions for an augmented program in the 1990s. Both of these involve the return of samples to Earth for detailed laboratory analysis, and both will make use of the Space Station capability expected to be available in the second half of the 1990s.

The first mission for the augmented program is to Mars. The SSEC recommends at least two flights, beginning in the mid 1990s, each of which includes a rover and a sample return module. This program would permit a wide-ranging, flexible exploration of the martian surface in combination with the return of intelligently selected and documented surface samples for analysis on Earth. Viking has provided us with adequate information to plan such a rover-and- sample-return mission, and candidate landing sites have already been selected. What is required is a will to proceed, and initial commitment of funds starting about FY 88 to develop the necessary technologies in semi-autonomous rover operations, sample acquisition, aerocapture, and other areas required for this mission.

The second mission of the augmented program is the return of a pristine sample of primitive material from the icy nucleus of a comet. This is in many ways a simpler mission in concept, and it is of comparable scientific priority to the Mars mission. It is also an excellent candidate for international cooperation, since a similar comet sample return concept has been selected by ESA in their strategy document Space Science Horizon 2000 as the cornerstone objective for European planetary science in the 1990s. However, the SSEC believes that a precurser comet rendezvous mission is required to provide scientific and engineering data needed to carry out comet nucleus sample return. This precurser mission is CRAF, the Core Program comet rendezvous requested for an FY 87 new start.

The SSEC has not yet published its recommendations for an augmented mission program, although they have been presented to NASA and were well received. Publication of these recommendations is expected in about six months, at which time the SSEC will have completed its task. Meanwhile, the members of the SSEC continue to be concerned that its recommendations for the Core Program are being implemented.

At present, it is my understanding that the planetary exploration program is making sound, solid progress. In January 1986 Voyager encounters Uranus. The long-awaited Galileo launch is anticipated in the spring, to be followed by the first spacecraft encounter with an asteroid--29 Amphitrite--in December of 1986.

Meanwhile, both the Venus Radar Mapper and the Mars Observer are on schedule and within budget. The management of the NASA planetary program should be congratulated for the disciplined and effective way they have implemented the SSEC recommendations. After a period of near disaster in the early 1980s, the U.S. is once again in the position to assume leadership in the scientific exploration of the solar system.

I must conclude, however, on a note of concern. It has recently been reported in *Aviation Week and Space Technology* that NASA management has decided not to seek an FY 87 new start for the comet rendezvous mission. If this is correct, it means the abandonment of the SSEC Core Program just as it is beginning. Essential to our strategy is the development of the Mariner Mark II spacecraft, taking advantage of the unique human talents available at JPL as the Galileo spacecraft development is completed. The Mariner Mark II is required not only for CRAF, but also for Cassini and subsequent missions. Further, a FY 87 new start is needed if CRAF is to provide the precuser data needed to plan later comet missions, including the sample return recommended in the SSEC augmented program.

A further consequence of failure by NASA to continue the SSEC Core Program is a breakdown in the scientific consultive process. The SSEC approach, with its emphasis on careful planning and cost-effectiveness, has been widely praised both within NASA and elsewhere. We have been quoted as an example for other scientific constituencies to follow. Such praise rings hollow if the SSEC plan is abandoned before it can begin to pay off in producing a more efficient and effective planetary exploration program. I urge this Subcommittee to examine carefully the FY 87 budget for Space Science when it is proposed by the Administration, and I ask your support for a continuation of the SSEC Core Program in FY 87, and for an initial investment in the augmented program in FY 88. Thank you.

Mr. **Nelson**. Which one of you has to leave?

[Dr. Morrison raises his hand.]

Dr. **Morrison**. But not for another 15 minutes.

Mr. **Nelson**. Oh, I understand.

Let me—well, let me just pick up right there what you said.

What do you think--you know, we're taking off tomorrow night for the Soviet Union. Dr. Paine is going to be with us; Jess Moore is going to be with us; Tom Stafford and Deke Slayton. We're going to have a 10th anniversary celebration with the cosmonauts, and so forth.

And we're going to throw everything on the table for discussion. What do you think about a joint unmanned mission where, as Dr. Sagan said yesterday, one nation would go out and rove around Mars and pick up the samples and the other one would come, land there, pick up the samples and take it back to Earth? What do you think about that?

Dr. **Allen**. We've looked at that, and I think it's a fruitful area for cooperation. It has a number of advantages. One is that—I think in the way you described it, as I understood that—it wouldn't involve a lot of direct hardware transfer, but only in the sense of Apollo-Soyuz, addressing some interfaces but not a lot of hardware transfer, which eases some of the problems that worry some people with regard to hardware collaboration. So that's certainly a possibility.

My own view is that I think that's very useful to talk about, and I hope that something comes of it, but at the same time there really are a number of near-term questions, too, that deal with our interaction with the Soviets and their planetary program that need to be addressed, I think, a little bit quicker than that.

Dr. **Morrison**. If I can amplify on that——

Mr. **Nelson**. Yes, please.

Dr. **Morrison**. Also, I think that we have had a very difficult time in dealing with the Soviets during the last few years because there has been an almost total breakdown of official contact, and, therefore, on both sides individual scientists have continued this communication, but under a certain amount of stress and difficulty. And we need to rebuild our bridges.

The kind of mission you envision is something we could do jointly, but meanwhile let's get together, also, and plan the data exchange on their Mars missions and our Mars missions that are taking place late in this decade on a stronger coupling of our programs, the investigation of Venus, and other areas, to build the trust and the experience which are necessary if we're going to undertake a mission of the sort that Dr. Sagan described.

Dr. **Allen**. I might make a few more general comments on the area of Soviet interaction.

Over the years the Soviet program has been a very active one. They have had about twice as many launches as we have in the planetary program. And it's been marked with some successes and some failures.

In the lunar days, you might recall, that they did some quite remarkable things. They put a rover down which roved around; they brought back samples in an automatic fashion from the Moon. The overall result of their program was very small compared to Apollo. In a sense, Apollo blew them away and there was no contest.

They made an intensive effort to investigate Mars, which was marked primarily by failure, and they had very few results of any consequence in their Martian emphasis. The United States, again with the Viking Program, certainly dominated the scientific results that have been returned from Mars.

On the other hand, they had a long-term and very successful program at Venus. Because of the success they've had with their landers and some of their other programs, in a significant measure the United States has deferred to that program and they have done a credible and competent job.

Nevertheless, even there where their programs have been quite successful, they have not fully achieved comparability with the West in terms of science. It's sometimes a little bit hard to know just why that is, because they have very good scientists and these missions have had a very high degree of technical success.

I think it's because they just simply don't have the depth in their scientific establishment. They're slow at getting data out. They don't publish it well. Their publications don't seem to be subjected to the same critical review that our own do, so they don't stand up very well in the total international regime of scientific inquiry.

There are great signs that that's changing. I think there is a new leadership in the Soviet Union. I think it's evidenced by people you'll meet—Sagdeyev and Velikov, Barsukov, and perhaps now with Gorbachev evidencing a different style of leadership, they will make different approaches of these things.

What we are seeing right at this moment is, in fact, new missions in which there is a great deal of Western collaboration, much more than there's been in the past. I think that's going to have the result of instigating better reporting, better scientific credibility at the international fora, and will generally raise the credibility of their scientific achievements.

Their mission to Halley, as you probably know, is really a marvelous tour de force; that is, they went to Venus; they put landers down on Venus with two spacecraft; put two balloons that floated at a constant altitude for a period of time in the Venusian atmosphere. The spacecraft have then gone on toward the intercept of Halley's comet. So that's, you know, really quite a remarkable technical achievement. How good the science is going to be remains to be seen.

The mission they're planning for Mars, which is called the Phobos mission, in which they'll rendezvous with the moon of Mars called Phobos, involves again a number of sophisticated technical things—rendezvousing, putting a lander down on Phobos, and all of that.

What's most interesting is that, when you look through at the scientific participants and the instruments and measurements that are on the Phobos mission, it is really a very competent array involving nations, many nations in Europe. And I think, therefore, the results on Phobos are going to be of quite high credibility.

These things are a little awkward for us because, of course, we had planned to go to Mars, a somewhat different mission, and, therefore, not directly competitive. But they'll be there several years ahead of us.

We also have on our long-range plan—not so long range, one of the next observers missions after Mars—a plan to revisit the Moon, but they have now just announced that they will go back to the Moon with a set of instruments which probably obviates the need for us to do the lunar orbiter as we would have planned to.

We're now looking at various ways that that mission could perhaps be reconfigured in a way that would be more compatible and mutually supportive with the Soviet program.

The Soviets just recently discussed an intention to do what we called Quasat; that is, an orbiting radio telescope which, when combined with Earth observations, will extend the baseline for making very precise radio telescopic observations. That was in our plan.

One gets the feeling that the Soviets, recognizing the particular competence of our academy structure in formulating objectives and strategies for investigating the solar system, have profited from that by picking out from the list of things we've identified as most important to do those which they can do; doing them substantially quicker than we can do them.

Now that's awkward. It causes many questions to be raised about our program. We need to find ways of accommodating to that better, either through cooperation, through better communication, through some way of having our programs be more rational. There is really no point in racing to do the same scientific observations.

Mr. Nelson. And you think this kind of cooperation can occur within a framework that can be mutually beneficial and at the same time protect each other's technology transfer?

Dr. Allen. There are obviously several kinds of cooperation. The first of those is just as you say—no attempt to mesh hardware in any way, simply agree in some manner to discuss and interlace programs in a way that are mutually supportive.

That probably is fairly easy to do. I don't know how easy. I don't know yet how open the Soviets are with all of their plans. They seem to be open, but they have surprised us in the past and they may be doing so now.

Certainly that's the easiest step. Steps that involve hardware collaboration clearly will raise these questions of technology transfer that are going to bother many people, but I think, even there, there are ways to do some hardware cooperation in which one can find ways to minimize that problem.

Dr. Morrison. I think it's also worth noting that the Soviets do have extensive collaborative agreements with the French in particular, but increasingly with other European countries on both sides of the Iron Curtain, and technology transfer and methods of doing cooperative work are going to be worked out there with or without us.

Mr. Nelson. Dr. Morrison, do you want to give us any comments on the space station vis-a-vis space science?

Dr. Morrison. Only in a narrow sense, Mr. Chairman. The specific—the core program in planetary science proposed does not depend on the space station in any way, nor would it significantly utilize it.

When we looked at the augmented program, which includes more technologically challenging missions, we found that, indeed, there is a close coupling with the space station; that both the Mars rover and sample return mission I referred to and the other one which I did not mention, which we have recommended, the comet sample return, need a launch capability which is significantly enhanced by using on-orbit refueling or assembly which could be done through a space station. And both of those missions, which involve sample return, could also profit by direct return of the sample to a space station laboratory.

We have, therefore, assumed that capability would exist and find that that would significantly enhance the ability we would have to carry out those missions, but, as I said, not for the missions of the core program.

Mr. Nelson. Before you have to take off, Dr. Morrison, given the fact that we're in budgetary tight times—maybe we're not if the White House and the Congress continue to spend at the rate of $200 billion more each year than we have, but that's just a political aside.

But, given that sooner or later the chickens come home to roost and we have to be in budgetary constraints, and if there are any slips, delays in some of these programs that the SSEC has recommended, do you think that the inevitable delays are really going to mess you up?

Dr. Morrison. I think that major delays would, precisely because we would run the danger that the cost of individual missions would rise as the intervals between them increase, simply because we would find ourselves having to duplicate activity, duplicate personnel. We would not be able to make efficient use of them.

What I would like to plead for is not so much the specific missions, one after another, laid out in the SSEC recommendation, although I think those are fine missions, but for the process that we have discussed—one that tries to plan ahead rationally, to let us think 4 or 5 years in advance how we're going to be using our people and our institutions, to allow us to make believable promises to the scientific community so that they can bring in graduate students and can get involved. So that if a slip occurs, as it may inevitably do, then we need to go back and reexamine that situation and reformulate our plan, but still keep the basic plan in mind.

What I hope we can avoid is going back to the all-or-nothing free-for-all in which it's just one mission competing against another. I think we will all make better use of our money and gain more scientific returns by a continuing process of careful strategic planning.

Mr. Nelson. The idea is that over the next number of years that we have four great observatories in the heavens: The x ray, the gamma ray, the visual and the Hubble, and——

Dr. Allen. The infrared.

Mr. Nelson [continuing]. The infrared. Now does that complete an array of observation that you all think is very significant for the expansion of knowledge of mankind?

Dr. **Morrison**. Speaking for myself, yes, sir; that is a splendid set of astronomical observing facilities and one that we should all support.

Mr. **Nelson**. Do you feel the same?

Dr. **Allen**. I, of course, agree. They're marvelous observatories. I think there are a couple of cautions that you need to add. One is that these instruments are general purpose instruments; that is, they are instruments to cover a broad region of the spectrum and to permit a lot of different experiments to be done by a lot of different observers. They won't completely obviate the need for special purpose measurements to be done from time to time by, hopefully, smaller and less expensive activities.

Mr. **Nelson**. And one such is the example—as you said, you put a telescope up there and——

Dr. **Allen**. Yes.

Mr. **Nelson** [continuing]. Block out the intense light?

Dr. **Allen**. Yes.

Mr. **Nelson**. You do that measurement from space instead of the ground?

Dr. **Allen**. Yes.

Dr. **Morrison**. Also, I think it's evident to everyone that those telescopes of those four observatories will be very useful for studying the planets, but that is not their primary purpose, and they in no way take the place of the kind of missions in which we send spacecraft to the planets themselves, which we have been focusing on in things like the SSEC recommendations.

Mr. **Nelson**. OK. Is there any further message that you would like us to take with us as we depart tomorrow night?

Dr. **Allen**. That's certainly an open invitation, Mr. Chairman. [Laughter.]

I presume you'd like to confine it more or less to the space science areas. [Laughter.]

Mr. **Nelson**. Feel free. [Laughter.]

Dr. **Allen**. I do think that in many ways the—there really is in my mind an urgency and an importance to figuring out how to deal with this question of cooperation in space and space science activities.

We are not looking good right now, in my opinion. We are doing missions in which they are jumping ahead of us and not doing exactly the same thing, and we discount it for quality on occasion, but, nevertheless, they're getting there ahead of time. That's silly. We'll look foolish after a time in that.

Mr. **Nelson**. Can you give me an example there?

Dr. **Allen**. Oh, yes, sir, we are doing the Venus radar mapper 4 or 5 years after they did the Venus radar mapper mission, and they may, indeed, do an intervening one also. Ours is necessary because it's of substantially higher quality and does the entire planet, but it, nevertheless, is just awkward that the United States is going with the mission and didn't really know that the Soviets were doing that Venus radar mapper until they were about on the way, and that's not a very good way to do business.

We have already planned our Mars observer. The SSEC Committee had made its recommendations. The United States had made a major commitment to that being the first of our observer series when the Soviets announced that they were doing their Phobos mission, which is—it's different, and, therefore, doesn't obviate the need for the Mars observer.

The lunar orbiter, as I mentioned, is even a little bit more awkward because that was the next one identified by Dr. Morrison's committee which is not committed to yet, but, nevertheless, next in our planning cycle. And that is almost duplicated by the mission which Barsukov announced the Soviets would do.

We'd placed a lot of emphasis on planning for this Quasat, the orbiting astronomical—radio astronomical observatory, and for them to announce it and then to solicit European support, collaboration with them, does not make us look very good.

So I think that we really have some choices to make. We can go it alone and just simply decide that we'll do things in areas where they really are not prepared or cannot do it, and, therefore, we'll be assured of our uniqueness. That's true certainly for Galileo and it will be true for comet rendezvous, I think. It would be true for the Cassini mission. But that is not a very satisfactory way to do it to me, either.

So I think we would be best served if we would find some way of—and I think cooperation is the right word, but at least mutually informing and rationalizing the efforts that we take. If we don't do that for our bilateral interest—that is, just to avoid stepping on each other's toes in this area—we're going to be forced to do it because of the importance to us of collaborating with the Europeans, and the European obvious higher degree of willingness to enter into these collaborative arrangements. So I think it's an urgent question that needs to be addressed directly and for which some answer needs to be found quickly.

Dr. **Morrison**. I strongly agree with that, and I would add that that is another answer to your question of what happens if, because of budget problems, some of these missions have to slip. If they slip or drop out and we have already entered into discussions or agreements with either the Europeans or the Soviets, that makes us look bad, too. And, again, this is a plea for a rational process, not an arbitrary one, so that we can be perceived as good partners for international collaboration.

Dr. **Allen**. The Soviets seem to be learning how to live in the Western World of PR, public relations, public perceptions of what they do. They seem to be learning that in the way they're approaching some of the arms control matters, and they seem to be working that pretty hard in the way that they do their scientific endeavors. And they're being rather successful at it. So we just need to recognize that.

Mr. **Nelson**. Dr. Morrison, you need to depart.

Dr. **Morrison**. Thank you very much.

Mr. **Nelson**. It's to that hour.

Dr. Allen, you were such a delightful host for this committee last February as we visited you, and one of the things that I have subsequently found out about JPL that I'm interested in I really didn't learn about that particular day. And that was you apparently have a program whereby your employees at JPL give of their time and talent to the local school systems. Could you describe that particular sharing arrangement for us here?

Dr. ALLEN. Well, we have several. I'm not entirely sure which one you're speaking of.

Mr. NELSON. I'm speaking of the elementary and secondary school systems.

Dr. ALLEN. Well, let me comment on several things we do.

First of all, we are members of the Adopt-A-School Program, in that we have adopted a local junior high school, and with that junior high school pay a great deal of attention to their science programs and provide—our employees provide a lot of assistance to teaching.

That junior high school—and I'm embarrassed that I don't remember the statistics exactly, but it basically walked away with the State science fair projects in the last year.

Mr. NELSON. Well, I'm sure it would with that kind of high-powered teaching. [Laughter.]

How in the world did you limit it to one?

Dr. ALLEN. Well, the Adopt-A-School Program is an organized one in which the employers of an area really are kind of assigned schools. We are assigned a junior high that is rather near JPL.

I've asked the same question you have: Can we extend it to others? To do that we'll kind of need to negotiate with the other employees; that is, this is a program that's having a good deal of success, and the other schools have been adopted by other people. But I think we could do more, and we're looking at that now.

We have a great deal of individual participation in the schools in people who sign up to assist with lectures, and we bring a body of teachers to JPL during the summer who participate in the laboratory in their field; that is, if they're science teachers or mathematics teachers, they work on the planetary programs in areas that relate to what they teach—a very valuable program.

We have a lot of summer students, both in minority programs and in undergraduate programs at the college level, who participate in projects during the summer.

I think some of the things that you may have heard about which have been quite interesting is that we have undertaken with the Los Angeles Unified School District to establish what's called a science and mathematics teachers' teaching resource center in the vicinity, part of the overall county's program which is called Teacher's Educational Computing Center.

In this particular center, with the help of some of the aerospace industries in the area, we have arrayed a very large number of personnel computers, a video studio with ability to do interactive work with disks and other forms of videotape material, and a wide range of resource material which is supplied by NASA dealing with all of the activities in which NASA is involved.

This center allows teachers—it works on teachers, where the leverage is the highest, to come in; learn themselves in programs which are arranged there, but also take back to their classroom programs which they have prepared on the computers and can be used on computers which they have access to there in their schoolrooms; videotapes to take back; slide shows to take back; various other kinds of materials which they can prepare to their own needs and take back to their classrooms to assist them in teaching.

We have had considerable success with this, have expectation of its being more successful, and it's being used as a model by some other activities to—as a way of getting a high leverage technical input into the schools.

Mr. **Nelson**. Have we had the second bells yet, the second set of bells?

Could one of you all find out for me what the vote is? Dial 5-7400.

So, in addition to the—to this center, you actually send employees of yours into the school system, but in this particular case you just adopted one particular junior high school?

Dr. **Allen**. There are a number of programs. We have adopted a school, and that we work very intensively.

A lot of employees on an individual basis are given the opportunity and the encouragement to go out and assist in the schools, giving guest lectures, bringing in experiments, and the like, to show in the classrooms.

And then we have this center.

Mr. **Nelson**. Now is this done under the auspices of Cal Tech or is this done in your capacity as a NASA center?

Dr. **Allen**. This is done in our capacity as a NASA center.

Mr. **Nelson**. Oh, it is?

Dr. **Allen**. Yes.

Mr. **Nelson**. I see. Very interesting.

Dr. **Allen**. I think we benefit from our association with Cal Tech and the dedication to the educational process, but these programs are programs that are encouraged by NASA. We have been asked in several of them to sort of set as a model for NASA, and we've helped NASA in what's—oh, I've forgotten the name of the program now, but there's a new program focusing on elementary schools in which we are assisting, being the lead in preparing course material for that.

Mr. **Nelson**. If we could get some details from your staff——

Dr. **Allen**. Fine. We'd be very pleased to do that.

Mr. **Nelson** [continuing]. It seems like to me that we ought to have this being done much more aggressively with each NASA center all across the country.

Dr. **Allen**. Well, I'd think you'd be pleased, also, if you asked what other centers are doing. I'm proud of what JPL is doing. I think we are the lead in many ways, but each of the NASA centers does have a program in which they work hard at this translation of their enthusiasm and knowledge to the schools.

Dr. **Soffen** [speaking from the audience]. Mr. Nelson, I'm Dr. Gerald Soffen of the Goddard Space Flight Center.

Since I represent that center, that particular program, we have a very parallel program, as do the other NASA centers. We look toward JPL because it carries a great deal of leadership, but I can assure you every one of the centers would have a representative like myself enthusiastic about those programs.

Mr. **Nelson**. What I'd like to do, Dr. Byerly, is at a later date let's get together a number of the center representatives and chat about this; get a good understanding of where—let's get the dialog started between each of them, how the success in one center might stimulate another center in their activities. Let's see what we can get going, because math and science in the school systems of the country is obviously extremely important.

Dr. **Allen**. Extremely important and badly in need of improvement. The United States is not looking good in terms of science and mathematics teaching in the grades K through 12.

Mr. **Nelson**. And what better resource than these centers of excellence which the NASA centers are?

Dr. **Allen**. Yes, sir; and I think you'll find that, although there doubtless is more we can do and should do, that we all take very seriously the responsibility as guardians of the taxpayers' money that the taxpayers' money go not only for science for science, but also for science for the excitement and education of the public.

[The information follows:]

Material requested for the record on page 104, line 2397, by Chairman Nelson on October 9, 1985.

JET PROPULSION LABORATORY EDUCATIONAL PROGRAMS

Background

Jet Propulsion Laboratory (JPL), along with NASA Headquarters and NASA Centers, is an informational resource for the educational community. This effort includes center vistor programs, speakers' bureau, and the distribution of publications upon request.

Under the auspices of the JPL Educational Outreach program, JPL formed partnerships with the educational community and assisted the Los Angeles County Department of Education in opening the Science and Mathematics Teaching Resource Center (SMTRC). At the same time, JPL embarked on a program to develop new curriculum materials utilizing computers, multi-media and high technology, aimed at the elementary grade levels. Emphasis was placed on teacher training. Expert educators were engaged as consultants in the areas of teaching methodology, evaluation, and audiovisual presentation. JPL supplied the expertise in space science and engineering content.

During this same period, NASA began "Operation Liftoff." Main points of this new program were the development of curriculum materials aimed at the elementary grades, establishment of a network of teacher resource rooms, seeking out partnerships, and the establishment of "lead centers" at each NASA Center for specific task management. JPL took leadership for the development of computer-based learning materials and associated audiovisuals.

Some Specifics of the Present JPL Program

The JPL Educational Outreach program is now housed in an 800 square foot Curriculum Development Center, close to the 5000 square foot SMTRC. JPL is field testing its products nationally. The first major item is "Microgravity?", an extensive computer-based module aimed at upper elementary and middle schools. This product contains teacher guides, student readers, and NASA reference materials in addition to the extensive computer software developed in partnership with small business. The module examines the basic laws of Kepler and Newton and the associated required math skills.

Similar products entitled "Robotics and Man," "Experimenting," and "Communications" will soon be tested. Each product contains components utilizing various media, e.g., Robotics and Man contains narrated film strips, such as "When Your Robot Gets Sick--A Billion Miles From Home" and "Is It An Alien Or A Rock?" in addition to teacher guides, student readers and software.

JPL has also developed Computer Search/Catalogs for NASA videotapes and printed materials available at the NASA teacher resource rooms. These allow teachers to search out videotapes and printed items based on subject content. At present, there are 95 different subject classifications such as biology, physics, careers, etc.

The materials developed by JPL are produced in accordance with objectives outlined in the California State Department of Education framework for science education. Periodic meetings are held with the Department to discuss the programs alignment with topics presented in the major textbooks and suggested teaching methodology. Teacher training and specialized workshops requested by the educational community are carried out.

JPL also seeks to work with other members of the educational and industrial communities, including working partnerships with universities, small companies, utility companies, publishers, and schools.

OPERATION LIFTOFF:
NASA'S ELEMENTARY EDUCATIONAL PROGRAM

Thank you for your interest in Operation Liftoff! This exciting new addition to NASA's Educational affairs Division has one important goal: to stimulate student's interest in science, mathematics and technology through NASA-related materials and services made avilable to elementary teachers and their students.

NASA will achieve this goal by serving as a catalyst in developing prototype concepts for educational materials and programs. We are looking to external organizations to design, produce and disseminate these curriculum support items via Teacher Resouce Rooms located at the NASA Centers. This dissemination point and reference room for all NASA-related educational materials is for use by educators, school administrators and curriculum specialists.

This combined commitment of NASA, the educational community and the private sector, is the foundation of Operation Liftoff.

To date, several projects are underway:

- Audio-visual: two videotape series and coordinated curriculum packages, centered around different aspects of space (food in space, spacesuits, computers).

- Computers and related audio-visual materials: software in basic science and space-related subjects Currently being developed: microgravity, Kepler's Law.

- Educational materials for elementary students: space and science information and activities, easily photocopied by the teacher, for the elementary comprehension level. Subject categories include: living in space, transportation, planetary/astronomy, and future in space. Also, a space science book (astronomy) is being produced for the primary grades by a commercial publisher.

- NASA information access system: a computer data base housing current and historical NASA information to be accessible via personal commputers for educator and student use. This system will be capable of communicating with a wide variety of personal computers.

- Teacher workshops: 2-weeks at the Kennedy Space Center. Pilot to be completed in Summer, 1985.

- Teacher intern program: an educational experience for one or two teachers at the Goddard Space Flight Center. Pilot begins in 1985.

- Other Operation Liftoff programs being planned: Preservice/In-service education; Graded School Programs; and Opportunity Conferences (for private sector and educational groups. First conference is planned for producers of education materials in Fall, 1985.)

These curriculum support materials will become available at the Teacher Resource Rooms and via other dissemination methods by September, 1985. We will announce updated information as it becomes available.

Operation Liftoff is interested in proposals for new educational methods or materials that can be categorized in the above program components. If you or your organization would like to submit a proposal, please send it to the program coordinator:

Ms. Debbie Jones
Operation Liftoff
Code LEE
NASA Headquarters
400 Maryland Ave., SW
Washington, DC 20546

May 1, 1985

Mr. **Nelson**. Thank you. We appreciate it very much.

The committee will recess, and we will resume the hearing tomorrow morning.

Dr. **Allen**. Mr. Chairman, my congratulations to you, and I'm envious very seldom of people, but I'm envious of you. I think that's marvelous.

Mr. **Nelson**. Well, you're very kind. Thank you, sir.

[Whereupon, at 4:24 p.m., the subcommittee recessed, to reconvene the following day, Oct. 10, 1985.]

SPACE SCIENCE: PAST, PRESENT AND FUTURE

THURSDAY, OCTOBER 10, 1985

House of Representatives,
Committee on Science and Technology,
Subcommittee on Space Science and Applications,
Washington, DC.

The subcommittee met, pursuant to notice, at 9:44 a.m., in room 2325 Rayburn House Office Building, Hon. Bill Nelson (chairman of the subcommittee) presiding.

Mr. Nelson. Good morning.

I am going to defer my opening statement so that I can yield to my colleague, Mr. Monson, who has a markup. He has got a number of important things he wants to share with us.

Mr. Monson. Mr. Chairman, I appreciate that very much. I am sorry that I won't be able to stay in this hearing today, because there are a lot of important things to be discussed, and there are a lot of things that are especially important to me and my State.

Lest anyone think that Utah is taking over today, maybe we're trying to. Senator Garn will be here, I understand, shortly. We also have with us today Scot Thomas, who is a senior at Utah State University, to share some of the experiences that he has had. He is one of the top students at that university, and the reason he is here is that he has had several experiments fly on the space shuttle and the get-away special program, and also he's had one of his experiments in the mid-deck.

One of these experiments flew twice, and there is another one that he has that they are anxious to have fly twice.

Mr. Chairman, if you're looking for a good experiment to take along with you, I might suggest that you consider Scot's experiment because I think you would find it to be an excellent one to participate in. That would be just about the right timing for it as well. So I would encourage you to consider that.

He received the Stoddard Space Scholarship this year, and we in Utah are extremely proud of Scot and his accomplishments and feel confident that he will continue to make significant strides in the space science field.

I want to welcome him to Washington and also to this subcommittee, and feel that, as I say, it's unfortunate that I cannot remain but there is a very important markup on the Superfund legislation that I must attend. I appreciate your sharing this time with me and giving me this chance to make these remarks this morning.

Thank you, Mr. Chairman.

Mr. Nelson. Thank you, Mr. Monson.

Mr. Monson is a valued member of this committee, and we are looking forward to visiting Utah at the first available opportunity that we can come out there and see some of the space activities that are resident in your congressional district. I am looking forward to that.

Well, we've had an exciting week. Today is the third in a series of hearings on the question of space science and applications, and today we are going to concentrate in the area of astronomy.

Dr. Field, who was the chairman of the Astronomy Survey Committee, will give us his views on astronomy based on findings of the Field Report.

Then, our panel is most interesting: Dr. Harwit, author of the book "Cosmic Discovery," will reveal how recent revolutionary discoveries came about and how these will affect the future.

One of our astronauts, Dr. Jeff Hoffman, an astronomer, is going to be introduced by his colleague and fellow crew member, one of our colleagues, Senator Jake Garn of Utah.

Then we are going to look at a glimpse to the next generation of scientists. Mr. Scot Thomas has already flown three experiments—as Mr. Monson has shared with us—in the area of fluid dynamics, and he is preparing his fourth. He worked at the Fermi Labs last summer, and he is an undergraduate at Utah State.

So I want to share my enthusiasm with Mr. Walker, my ranking member, and any comments you would like to make.

Mr. **Walker**. Thank you, Mr. Chairman. I have no comments. I congratulate you for putting together this series of hearings. We have had a lot of interesting testimony over the last several days on this bill on space science. I think that today's testimony will also help build upon that record. So I thank you and look forward to the hearing.

Mr. **Nelson**. Dr. Field, if you would share with us, you have been sitting through these hearings almost as a participant, because everybody would keep turning to you asking your opinion even though you were sitting back there in the audience. So as you know, we would prefer that for each one of you your written testimony will be a part of the record, and rather than you read it, we would like for you just to share with us, and we will conduct this in a fairly informal manner.

Dr. Field?

STATEMENT OF DR. GEORGE FIELD, CENTER FOR ASTROPHYSICS, 60 GARDEN STREET, CAMBRIDGE, MA

Dr. **Field**. Thank you, Mr. Nelson.

Indeed I have been here throughout the hearings. Not only did I find them personally fascinating, but as a member of the National Commission on Space, very frankly, I am trying to make sure that the commission report reflects the very latest and best thinking of our space science community. I share your thoughts that the witnesses that we have had are among the very top people that could have been obtained to testify here.

I want to thank you, particularly, and the staff for making this opportunity available, not only the hearings that we have been having this week, but also the informal conference, which I think has been very fruitful.

You know that we scientists depend completely on the Congress, the understanding of the Congress in forwarding the scientific program of the United States, and we realize that it is not always easy for people without a serious technical or scientific background to deal with this, and we therefore appreciate your efforts on our behalf.

Let me start my statement by making a simple point that was made by Jeff Hoffman, one of my colleagues in the discussion, I think yesterday, that almost all children are fascinated with the stars, and astronomers are just children who never grew up.

But I think that many people share that fascination throughout their adult lives, and I suspect that some of you have chosen to serve on this committee because of your fascination with space. And somehow I think our task is to communicate the excitement of exploration to our community as a whole, and I think this hearing will hopefully help in that direction.

As I have been listening to the hearings that we've had on planetary science and on Earth science, on other branches of space science, I have been struck that space exploration is really a seamless web. It is not really divided as we conveniently do in a bureaucratic fashion into planetary exploration, lunar exploration, study of the Earth, study of the stars, and so on. We are all engaged in an enterprise to understand the universe in which we live, and there are many overlapping areas of interest. I will mention a specific one in a moment.

But I think within that context you can think of astronomy as commissioned, if you will, to study the most distant horizon in the universe, to construct the most powerful instruments, and to see the most distant objects which we can observe.

Others, like the planetary astronomers—I think Carl Sagan used the phrase—are engaged in an exploration of the solar system in the spirit of "looking over the next hill." We too are exploring the universe. It just happens that the next hill is about a billion light-years away.

Others, like Vern Soumi, whom you heard yesterday, coined the phrase, I think it is, "of looking downward while looking forward," and he and his colleagues are using space to look back at the Earth and to find out fundamental information about the Earth as a planet.

We are all engaged in the same exploration, and there are many interconnections between the fields. An example that I would call attention to is the forthcoming Comet Halley. Public excitement is building about this encounter which we are having, a once-in-a-lifetime opportunity, next year.

But the fact is that everything we know about comets, at least up until last month, has been based on astronomical observations. We know a lot. Now, however, we are entering an era when interplanetary probes will go to the comets, and that, of course, is a highlight of what we expect in March 1986—so, from astronomy to planetary exploration.

Finally, we are learning that comets are possibly quite relevant to the history of the Earth and even life on Earth. I am speaking, of course, of the hypothesis that cometary impacts have interrupted the flow of the evolution of life on Earth and possibly are responsible for the extinction of the dinosaurs.

I would like now to mention just a few discoveries that have happened in the last few months that I think are of extraordinary importance. I won't take the time now to show the viewgraphs, but several witnesses have spoken of this fascinating object called Beta Pictoris. It's a normal star that we didn't think was terribly interesting, but the infrared astronomy satellite IRAS discovered that it had an anomalous infrared emission, and optical astronomers are now scrambling to catch up with Beta Pictoris and other stars of that character that were discovered to be anomalous by IRAS.

What has been found, as was described by Dr. Lew Allen yesterday, is that there is a disk of dust around that star which is strongly indicated to be made of particles which are of the type that could well have been the prelude or the precursors of planets within our own system.

I would like to call attention to the fact that theoretical models have anticipated this discovery. We are not simply groping in the dark. There are theorists who have been working on this problem for 20 years or more and who have, in fact, posited that such a disk must exist somewhere in the galaxy, and now we're finding them. Later I will just briefly show an image of that which I think is superior to most that you have seen.

Another object that I learned about, I think only 2 weeks ago, is one discovered by Spinrad and Dzorworski at the University of California-Berkeley. They used a new technique to observe a galaxy which is by far the most distant galaxy ever observed, and because of the time that it takes light to travel from that galaxy, the age of the universe when the light left that galaxy was only about 15 percent of the total age of the universe now.

In other words, at the time that the light left the galaxy which we are observing, all the processes which led in our own galaxy to the formation of planetary systems like our own, and possibly Beta Pictoris, had not yet had time to occur. We are looking back into the past in a kind of archaelogical dig into the phenomena that may have preceded the origin of the planets in our own galaxy.

Another discovery was made by Forman and Jones, who have been using the x-ray data accumulated by the High Energy Observatory, which is referred to as the Einstein X-ray Observatory. It flew in 1978 to 1981. We will have occasion to discuss later what the follow-up to that should be.

But using those data files, they have found remarkably, that a certain type of galaxy called an elliptical galaxy, which is common throughout the universe, almost invariably is surrounded by a halo of material which never was observed by optical astronomers and could only be observed in x rays because it emits radiation due to a hot gas which does not emit visible radiation very well.

Measuring this glow of x-rays, they have proved that these galaxies contain 10 to 100 times more matter in them that we thought existed. That is to say, the visible light from these galaxies does not give a true indication of their total weight.

Thus, we think there must be some kind of mass in the universe in these galaxies—it's not the gas itself, but something else—which is holding the gas in the galaxy and which is itself invisible. And the evidence is accumulating that much of the mass in the universe is invisible, and suggestions as to its nature and cause are of great interest at the present time.

Now let me turn to the Astronomy Survey Committee report. I think you are all familiar with it. It's been out for 3 years now. But just to make some comments on its implemenation.

I do support the planning activity which NASA has undertaken, particularly that that was described to you yesterday by Dr. David Morrison, particularly the Solar System Exploration Committee, which he chairs.

He made the valid point that in order to have an effective program of planetary exploration, it is necessary to have new starts on a regular basis. I can only support that in the planetary field, and I think it's important to bear in mind as we look at the Astronomy Survey Committee as well.

Let me recall to you some of the highlights of Astronomy Survey Committee Report. The very first item of discussion was a recommendation to complete the program which had been recommended in the Greenstein Report in 1972. That program included the Gamma Ray Observatory, the Hubble space telescope, the solar optical telescope, and what was then called the shuttle infrared telescope facility. We call them SOT and SIRTF, for short, the last two.

You are well aware that GRO and HST—the Hubble space telescope—are in development at the present time, and we anticipate that they will form the cornerstone of the sequence of great observatories which will be discussed at greater length by my colleague, Dr. Harwit.

But I want to remind the committee that the solar optical telescope still has not received funding. It's still under study. And this is a program which is of importance and which was given priority by the Astronomy Survey Committee. We still would support that mission as a crucial element in the development of solar physics.

With regard to SIRTF, you know that the space agency has reconfigured this mission from what was intended originally to be a short-term shuttle mission to a free-flyer of what we will now refer to as the great observatory class. And unfortunately, only very modest study funds have been made available to carry out SIRTF up to the present time, even though that was certainly being considered 5 or 6 years ago as a sure thing.

But getting to the major new programs, the highest priority that was recommended by the Astronomy Survey Committee was given to the then flourishing science of x-ray astronomy, invented in the United States, developed brilliantly by groups around the country, first with the Uhuru experiment, then with the SAS experiments, and finally the Einstein X-ray Observatory. Momentous discoveries were made in astronomy, and we felt very strongly that it was important to follow up with a permanent observatory of the same scope as the Hubble space telescope.

We had therefore recommended the advanced x-ray astrophysics facility, or AXAF, and I think you know the history there. I can only say that Dr. Hinners in his presentation made the point that he recalls telling one of our colleagues in 1980 that he was very disappointed to have to tell him that it had been shifted from fiscal year 1980 to fiscal year 1981. We are now looking at 1987, and Aviation Week tells us that it probably won't make it in 1987. We are very concerned.

Mr. **Nelson**. Of course, with the Hubble, we were expecting it to be a lot earlier than it is.

Dr. **Field**. Yes. We can speak later, Mr. Nelson, about the logjam that has occurred as a result of the Hubble space telescope.

Now, on the more moderate missions, I am pleased to report that significant progress has been made. In particular, a significant augmentation has been made to the explorer budget. The experiment now known as Fuse——

Mr. **Nelson**. Dr. Field.

Dr. **Field**. Yes.

Mr. **Nelson**. Let me interrupt you here.

Dr. **Field**. Surely.

Mr. **Nelson**. My colleague is here, and out of congressional courtesy, I want to yield to him.

The Senate is working today on a little thing called "the debt limitation crisis." [Laughter.]

Dr. **Field**. I understand.

Mr. **Nelson**. And we need to give him the luxury of returning to wage those battles.

Senator, you honor us with your presence.

STATEMENT OF HON. JAKE GARN, A U.S. SENATOR FROM THE STATE OF UTAH

Senator **Garn**. Thank you, Mr. Chairman.

Although I do have to return, I am not sure that anything significant is going to happen in the Senate. We have been talking for more than a week, and as I passed by the Chamber to come over here, the hot air was stifling as it came out the main door into the hallway or the foyer. [Laughter.]

But I do appreciate the opportunity of being with you today, for a very particular purpose: To introduce a very distinguished scientist, Dr. Jeff Hoffman.

I might say I know him very well. I had the opportunity to fly with him on STS 51-D, and I don't know that anybody has watched him as closely as I have in the performance of his duties.

I think it shows the unique nature and opportunities of the space shuttle, where we have gone from the early days of flight 4 years ago when Bob Crippen and John Young were test pilots of the "leather helmet and silk scarf" variety that go up and test them, to evolving beyond the test stage to the mission specialist stage, where people like Jeff, with a distinguished educational background in astronomy can become astronauts and be able to use their particular knowledge in scientific endeavors.

I might just say that as you and I both get involved in the arguments over the commercialization of space and pricing policies for delivery of commercial satellites, I would say that I am not very much worried about how much we charge for deployment of satellites in space—and the reason I say that, in our competition with France particularly in the ELV and the subsidization that France always gets involved in, whether it's airbuses as opposed to Boeing and their predatory financing and all of that, I think we make a terrible mistake if we worry very much about charging full price for delivery of a particular satellite into orbit.

My attitude is, whatever we are able to get from the commercial sector as an offset of the taxpayers' money being used in space is a bonus. But if we go to full cost, I will guarantee you all we will do is guarantee that Ariane puts all of the satellites into space and we will have no offsets whatsoever.

I personally can justify the operation of the shuttle on scientific means alone. The investment that we put into space is relatively small, as I have said many times when people ask me the question, "Why do we waste money in space?" We have never wasted any money in space. I did not take a credit card or a wallet with me. There are no stores out there. We have never bought anything out there. It's all right here on Earth.

I am certainly not going to take the time of this subcommittee to talk about spinoffs—they are almost without number—of things that we use every day here on Earth that benefit all mankind as a result of space technology.

So you have a distinguished group of scientists before you today, and I can justify $7.5 billion a year. When I think of another member of our crew, Jeff's and my crew, going on with the electrophoresis, I don't know that Charlie Walker or McDonnell Douglas are going to be the ones that come up with a cure or a preventative for some major disease, but I do know that will happen in zero gravity. There is no doubt about it at all. And when that does happen, we have justified the entire cost of every dime we have ever spent on space in the medical field alone.

When you look at what we will learn about our own origins, when we get the space telescope up, and what these gentlemen are talking about, what we learn about the universe will be justified. There are just so many areas.

So I don't worry a great deal about how much we charge to put a syncom out in space. Well, excuse me for diverting to that.

But Jeff is a close personal friend of mine, having had the opportunity to train with him for 3 months and to see how magnificently he performed in space. If you don't know, you all should know that he participated in the first unplanned extravehicular activity in the history of the space program, and to watch him and the rest of my crew rehearse for several months and you do everything over and over again individually in the simulator, and then you practice them in groups.

As you will find out, Congressman, you have integrated simulations and long sims, and to watch a crew—and I have watched a lot of them over the years—do so well and perform so professionally is one thing, but then we had two days of our mission that was not planned at all. We had no crew action plan, we had no checklists.

Yet as I watched Jeff go outside—and it was almost impossible to believe that I was sitting there watching him outside the spacecraft all by himself, trying to attach flyswatters and lacrosse sticks to the end of the end effector—but still I didn't notice any difference. I didn't notice any difference in the way he performed and all the things he had practiced or all these things that we had never heard of and did not anticipate.

I think that is a real tribute to Jeff, to see the professional nature in which he performed. It was a magnificent performance, and I am just proud to be able to have that friendship, to be able to see how he worked. He's a credit to his profession and his background. And I just very much appreciate the opportunity of you letting me say that here today.

Thank you very much.

Mr. **Nelson**. Well, it's a pleasure. And it's doubly delightful for you to be here today because we have one of your constituents here also on this panel, Scot Thomas, who is a student at Utah State, who has designed a number of the experiments in fluid dynamics, has flown on three shuttle missions, and he's preparing a fourth. So we are just delighted that you could come.

Senator **Garn**. I was up at Utah State University in August to go through all of the experiments that they're working on. There is another distinguished Utahan sitting in the crowd, Gil Moore, who has spent his life involved in the space program. We are well represented here today.

Mr. **Nelson**. Well, it's a testimony to the camaraderie of your crew that you would come over here especially to introduce Dr. Hoffman. We appreciate that so very much.

Senator **Garn**. Thank you, Mr. Chairman.

Mr. **Nelson**. Good luck today, and send us a good package. [Laughter.]

Senator **Garn**. Eventually.

Mr. **Nelson**. Dr. Field, thank you very much for allowing me the courtesy of introducing the Senator and having him make his remarks.

Dr. **Field**. Not at all, Mr. Nelson.

Mr. **Nelson**. Yes, sir. Please proceed.

Dr. **Field**. I was talking briefly about the moderate missions which were recommended by the Astronomy Survey Committee, and I mentioned that the committee had recommended an augmentation to the Explorer program. NASA has, in fact, accomplished that.

It has under study a mission called FUSE, or far-ultraviolet spectroscopic explorer, which we also support strongly.

It also recommended—and this may have been somewhat overlooked—a mission called space VLBI. That mission has taken a new name, called Quasat, and under that new name I think we may have some discussion later. Let me only say at this point that the planning has gone ahead with that, and there are opportunities for international collaboration which we might want to explore.

There were two other things that we discussed, one of them is called the advanced solar observatory. This was a facility which would use as its core the solar optical telescope, which would initially be in a visible ultraviolet region but then would be augmented by x-ray and gamma ray instruments.

I have to say that we are quite concerned that that program has not been able to proceed because, as you know, the solar optical telescope, or SOT, as I mentioned earlier has not received adequate funding.

A number of cosmic ray experiments were recommended also.

Finally, the search for extraterrestrial intelligence; if you recall, our committee was perhaps the first of the Academy to make a definite recommendation in that area, and I am pleased to say that NASA has proceeded and that activity is moving into high gear there.

Well, just to summarize then, I would like to say that we are concerned about a number of issues. Perhaps we can get into them in the question period. But we are also looking ahead. I think your committee is aware of a Space Science Board activity called "Major Directions in Space from 1995 to 2015"; that is, in terms of the planning cycle, it extends roughly 20 years into the future beyond what we could do at the present time.

Without going into detail, Prof. Bernard Burke, chairman of that committee—rather, the Committee on Astronomy that is part of that activity, the overall committee being chaired by Dr. Donahue, whom you know—with a number of experts has found that the probable direction in which we will evolve in astronomy is toward the use of what we call the principle of interferometry, in which one uses two or more telescopes together and joining the beams from those telescopes in one way or another to facilitate the attainment of extremely high angular resolution.

We have been discussing angular resolutions between 10 to the minus 3 arc-seconds and 10 to the minus sixth, a thousandth and a millionth of an arc-second.

Now, an arc-second is not something that everybody thinks about everyday. It's the size of a dime as seen from a distance of a mile. So we're talking about, you know, not only reading the fine print on the dime, but doing so from across the United States.

These facilities can be built. We have some preliminary designs on the table. And I think you will find when those proposals come before you, that they are scientifically very exciting.

Finally, I would just like to remark that I think at this time the astronomical community in its relation to the space program regards as the very highest priority the completion of the great observatories program, but we request also that you not lose sight of some of these moderate missions and, in particular, the solar optical telescope and the space VLBI.

Thank you, sir.

[The prepard statement of Dr. Field follows:]

THE FUTURE OF SPACE ASTRONOMY

A Statement to the House Subcommittee on Space Science and Applications
October 10, 1985

by

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Introduction

Future prospects for astronomy are exciting. New discoveries, including new objects which may be black holes, giant voids in the universe, gravitational lenses in space, and dust clouds near stars where new planets may be forming, challenge our understanding. Stars, planets, and galaxies are now understood as a result of the evolution of the universe, from its creation in a big-bang explosion 10 billion years ago, to the emergence of man millions of years ago. Today each new discovery can be used to test and to enlarge our emerging comprehension of the structure and evolution of the universe.

The scientific community as a whole has supported this intellectual enterprise becauses it is directed at fundamental questions which relate to many branches of science, particularly physics, earth and planetary sciences, and to a degree, life science.

The connections with physics are very close. Because physics is the search for general principles which govern the structure and interactions of matter, and because conditions which can be found in the universe (large distances, strong magnetic and gravitational fields, very dense and/or hot regions), principles of physics can be tested in environments which are inaccessible in the laboratory. Examples include nuclear fusion in stars, the transition to superfluid state at nuclear densities in neutron stars, singularities of gravitational fields in stellar black holes, and phase transitions in very hot matter in the early universe.

Earth and planetary sciences are directed at understanding the formation and evolution of the earth and planets. Astronomical observations of other planets in the solar system contribute to this enterprise, as do observations of dust-disks around other stars which may be regions where planets will form in the future. The study of organic molecules in interstellar space gives clues to chemical processes which are believed to have preceded life on earth.

The Role of Space Research

Until the late 1950's, all astronomical research relied on observations made with optical and radio telescopes on the ground. With the establishment of NASA in 1958, the ability of aircraft, balloons, rockets and satellites to carry astronomical instruments above the atmosphere has revolutionized astronomical techniques. For the first time in history we can study radiations which are blocked off by the atmosphere (gamma-ray, x-ray, ultraviolet, and infrared), as well as improve the sensitivity of observations at optical and radio wavelengths. Space astronomy supported by NASA is fully integrated with ground-based astronomy, largely supported by NSF.

Astronomy Survey Committee

Since 1964 the U.S. astronomical community has presented its needs to the government in three Survey Reports. I was privileged to chair the Survey Committee whose 1982 Report, *Astronomy and Astrophysics for the 1980's*, recommended facilities and programs for the current decade. I will summarize some of the main conclusions of the Report, particularly as they relate to NASA.

The Committee recommended that a balanced program of space and ground-based astronomy be carried out in the 1980's, at a cost not significantly greater than the program for the 1970's. Completion of the program recommended in the 1972 report, including the Gamma-Ray Observatory (GRO) and the Hubble Space Telescope (HST), was also urged. HST and GRO are now under development, and scheduled for launch in 1986 and 1987, respectively. We also recommended that the Solar Optical Telescope (SOT) and the Space Infrared Telescope Facility (SIRTF) be completed as part of the Spacelab program. Both programs have been seriously delayed, causing serious concern.

The highest priority for major new programs is a 1.2-meter instrument for x-ray astronomy, the Advanced X-Ray Astrophysics Facility (AXAF); it is now in phase B in preparation for a Fiscal 87 new start, but we are very concerned that this may not be implemented, according to present NASA plans. X-ray astronomy was initiated in the U.S., and a number of x-ray missions have yielded extraordinary results. If good young people are to be attracted to this field, it is urgent that AXAF be started now. A major new space astronomy program of lower priority, a Large Deployable Reflector (LDR) for observations in the far infrared range, is now being studied for implementation in the 1990's. In the moderate mission category, we are pleased that NASA has substantially augmented the *Explorer* program, enabling COBE, EUVE, and XTE to proceed and new missions to be studied, following on the sensational success of the Infrared Astronomy Satellite (IRAS) *Explorer* in 1983. Studies of FUSE are proceeding as planned, as are those of QUASAT, a series of cosmic-ray experiments, and a Search for Extraterrestrial Intelligence.

The Era of Great Observatories

NASA has reconfigured the original SIRTF from a sortie mission (with limited-duration flights from the Shuttle) to a permanent observatory in space like HST. The current plans thus include major observatories operating at gamma-ray (GRO), x-ray (AXAF), ultraviolet and visible (HST), and infrared (SIRTF) wavelengths, making possible a coordinated assault on astronomical problems across the entire electromagnetic spectrum. This program, complemented by optical-infrared and radio ground-based instruments already in operation or being planned for funding by NSF, will enable problems to be studied with great power.

Scientific Problems

I will mention only a few of the dozens of problems to be attacked by the major observatories.

IRAS revealed dozens of stars which are emitting an unexpectedly large amount of infrared radiation. Optical study of one of them - Beta Pictoris - revealed that it is being orbited by a disk of dust which absorbs radiation from the star and reradiates it in the infrared. Such a dust disk is believed to have been the precursor of planets in our own solar system. Could there be planets orbiting Beta Pictoris and other stars? SIRTF will use the infrared energy emitted by planets to image them if they accompany the nearest stars. Discovery of such planets would have important implications for the study of our own solar system, and would provide important data for SETI.

Newly-forming galaxies at the edge of the observable universe are receding so fast that their emission is doppler-shifted into the infrared range, where it can be studied by SIRTF if the light left the galaxies when the universe was 4% or more of its present age. AXAF will discover such galaxies via the emission from the billion-degree gases heated by stellar explosions expected in a young galaxy. Current theories predict that galaxies were born in the period observable by AXAF and SIRTF, so such observations are a critical test. HST has a role to play as well. Because it is above the atmosphere, which blurs the images of galaxies, HST will obtain sharp images of very distant galaxies, enabling us to study their evolution.

Finally, particle physicists seek a unified theory of the four forces which operate in the physical world. They predict that at a very early epoch the universe went through a period of extremely rapid expansion (called "inflation"), resulting in a unique dynamical balance in the universe today. If this is correct, 90% of the matter in the universe is in some strange form, such as photinos or axions, which are invisible but which still interact gravitationally. Astronomers have discovered that there is indeed much invisible matter in the universe, conceivably of the strange form predicted by particle physics. AXAF will discover very hot gas around galaxies, whose distribution reflects their gravitational fields and hence the amount of invisible mass in them. HST will enable a better measurement of the rate of expansion of the universe, which is critical to determining the amount of invisible mass.

Thus, the great observatories - GRO, HST, AXAF, and SIRTF - will make a comprehensive attack on far-reaching problems in astronomy. All of the great observatories should be in orbit during the same time period, to maximize the synergy which will come from their simultaneous use.

Beyond the 1980's

The Space Science Board of the National Academy is now studying future directions in space science. Its work is not yet complete, but it is already clear that the Space Station will open up dramatic new opportunities for astronomy by enabling astronauts to assemble very large facilities in orbit. This enables us to consider telescope arrays which will have unprecedented angular resolution. For example, an array of eight 8-meter telescopes operating in the range between 1000 Angstroms in the ultraviolet and 30 microns in the infrared could resolve details smaller than 0.01 arcseconds and would enable us to study objects 100 times fainter than ever before observed.

Summary

Space astronomy stands on the threshold of an unprecedented era of discovery. Implementation of the high-priority AXAF and SIRTF programs will complete an array of four great observatories to operate in the 1980's and '90's, enabling us to attack major problems in astronomy. Plans for the following period are based on the fact that the Space Station will make it possible to construct in orbit more powerful facilities. We hope that Congress will approve the program of great observatories now, and provide adequate funding on a timely basis.

Mr. **Nelson**. Thank you.

You remember some of the questions that were posed in the last couple of days, and one of the gentlemen immediately turned to you in the audience to get you to explain. I was chairing. Our committee had gone up to Bell Labs and had talked to Dr. Penzias, and without me having spent very much time in this area, I am quite intrigued as to how Dr. Penzias' theory that we are sensing this radiation that is being emitted and it is emitted from, according to his theory, from the very beginning in the "big bang."

Now, can you explain that to me?

Dr. **Field**. Mr. Nelson——

[Laughter.]

Dr. **Field** [continuing]. Since that moment when he for some reason turned to me, I have given it further thought to how to explain it, so I will offer another interpretation.

In fact, his interpretation that we're dealing with a hot material which suddenly expands, just as in, let's say, your refrigerator, the same principle is used. The gas will cool off, and that's how a refrigerator works. And we're living in a "cosmic refrigerator," if you will, an expanding universe in which the gas is cooling off. I think that is the correct explanation.

There is another way of looking at it that I would just like to throw out. It's kind of interesting. And they are all equivalent, it turns out, these different ways of looking at it. Long ago and far away, the universe was very hot, and we talk about millions or even billions of degrees, but at a particular moment the temperature fell to about the surface temperature of the Sun, about a few thousand degrees.

Now, imagine, if you will, the whole sky covered with a surface as hot as that of the Sun. Surely if the whole sky were as hot as the Sun, we would be fried to death. The Earth would be in very serious trouble.

The gas in the universe at the time that the radiation which Dr. Penzias sees left it, was in fact at the same temperature as the surface of the Sun. So, in a sense, we are surrounded by such a surface. The difference is that that surface is receding from us at 99.9999 percent of the speed of light. That's because it's very far away and the law of the expansion of the universe is that the further away it is, the faster it's going.

It's the same effect that was used by Dr. Spinrad and Dr. Dzorworski in identifying this galaxy which is at such a great distance. They measured the so-called red shift.

So, in summary, we are surrounded by a kind of stellar surface, if you will, shining everywhere, but it is receding at such an enormous velocity that the radiation which it emits is shifted all the way down into the radio regime, where it looks like it's only 3 degrees instead of 3,000 degrees.

Mr. **Nelson**. I see. When you say it's receding, do you mean the stellar surface is expanding?

Dr. **Field**. Yes.

Mr. **Nelson**. Still further? So it's going away from us?

Dr. **Field**. It's going away from us.

Mr. **Nelson**. I see.

Dr. **Field**. As a consequence of the expansion of the universe.

Mr. NELSON. Well, this certainly wasn't the purpose of our hearing for you to explain this to me, but I am absolutely fascinated by this.

Now, earlier in your testimony you talked about that we may now have discovered the most distant galaxy, and I think you said 14.5 billion light-years. All right. Now, where under Penzias' theory does discovery of this new galaxy, how does that fit?

Dr. FIELD. Yes. I would be happy to try to explain that. The radiation which Penzias has seen left the surface, which I was speaking of, when the universe was one-thousandth of its present size. The radiation which is coming from the galaxy which was just discovered left it when the universe is one-quarter of its present size.

So, if you will, there is a vast desert of information between those two eras, the very early times when the universe was a thousandth of its present size, and a quarter of its present size. The point is that observations of the type that astronomers can make of structures and details have been pushing out further and further and the record is now held by this particular galaxy.

The kind of radiation that Dr. Penzias observed is not from any structure. It's from the universe itself. And it conveys some kinds of information, too, but it's not the same as the kind of astronomical information that we're seeking.

Mr. NELSON. Let me pick up on a question that I asked yesterday. Dr. Lew Allen—General Allen—was here, and he had the photograph of the discovery of what we think might be another solar system by the special telescope on Earth that blocked out an enormous concentration of light that showed the little ring, what we might interpret as a solar system.

I asked the question, and I didn't understand the answer. Was that solar system, if it indeed is, was it in the same plane as our own solar system?

Dr. FIELD. Right.

Mr. NELSON. And he said, "Probably not." And I didn't understand why. Why, if there is a great order to this entire thing, would it might not be in the same plane as our solar system?

Dr. FIELD. I was fascinated by your question and the fact that he didn't know the answer, because it says something about the way astronomers and scientists think. The fact is that your question was a very good one, and if it should turn out—and it will be a simple matter to look it up; I haven't had the chance to do that—that it were in the same plane, then indeed there is an order in our galaxy which is somewhat unexpected. And I think I can explain why.

We see rotation everywhere. All of the stars are rotating. There are different systems of stars that seem to be rotating, one star around the other. We call them "binary stars." And for years people have been fascinated with the question, "Do their orbits line up?" They don't. And therefore, I think it was within that framework that he probably answered as he did, "It would have been surprising, because of our information about the stars."

Mr. Nelson, if I may, I would like to take the opportunity to show yet another image of this object. It was made by one of his own scientists, and I think is even more recent than the image that he showed.

Mr. NELSON. Sure.

Dr. FIELD. It's very difficult to see here, but it's really the same kind of image but in black-and-white. Let's see if I can sharpen it a little bit.

This is truly an extraordinary achievement to have been able to do this. As he explained yesterday, this is a star, Beta Pictoris, and it's hiding behind a little disk, and the telescope frame hides its light, which is 100 million times brighter than this protrusion that we see here.

I call attention to the fact that this protrusion is more finely delineated in this particular image. This is the very most recent image made by Brad Smith and Richard Terrile, who did the work.

You see the evidence that perhaps we are dealing with a disk of material here, much like perhaps that of the solar system. This dimension here is comparable to the entire dimension of the solar system. If the Earth—if an Earth were present in orbit around this star, it would be hidden behind this disk. Therefore, much further work will be necessary if we are to observe the disk itself. And I think that's what he was referring to—I am sorry, not the disk itself, but rather any planets that might be there.

The current hypothesis is that perhaps the light is fainter here, that the dust which is causing this part of the disk has already coagulated into planets, closer to the star, and, therefore, there is some hope of finding those planets.

Mr. Nelson, I think it does demonstrate again the seamless web that I was talking about because the planetary scientists are claiming this for their discovery, the astronomers regard it as astronomy. There is really no significant difference. We are all interested in the same issues.

Mr. NELSON. Do you see the four great observatories that we are going to launch as being the major step then for us—the major next step—for us then in understanding of the universe?

Dr. FIELD. Yes, I do, sir. They will have unparalleled power. I think I should not take the time of Dr. Harwit to go into the details here.

But we believe that the presence of man in space—to come back to a subject that has come up several times in the hearings—will make it possible for us to operate these observatories over the long-term. Without that, it would be impossible because surely there will be breakdown of instruments, a requirement for refurbishment of instruments, and replacement of expendables, and also from time to time putting new instruments at the focus of these telescopes. It makes sense to operate these using man in space.

In the longer term, we look forward to the advent of the space station because that will also permit us to construct facilities in space. That goes beyond the great observatories program, however.

Mr. NELSON. Does your time permit you to stay on?

Dr. FIELD. Yes.

Mr. NELSON. For the duration of this morning?

Dr. FIELD. Yes, I would be happy to, sure.

Mr. NELSON. I am going to turn then to Dr. Harwit, and we will just continue our discussion.

Let me just ask one other question. Could the star—right there in the picture—could it rotate on the same intergalactic plane?

Dr. **Field**. Oh, yes, I am sorry, I don't think I have really answered your question. Yes, it could in principle, but I would have to answer, as Dr. Allen did before me, that I don't know whether it does or not. Because of the reasons I explained, I think it would be surprising if it did.

Mr. **Nelson**. Dr. Harwit.

Mr. **Harwit**. Do you mind if I talk from over here?

Mr. **Nelson**. At your pleasure.

STATEMENT OF MARTIN HARWIT, CORNELL UNIVERSITY, ITHACA, NY

Mr. **Harwit**. I scurried back to my suitcase a minute ago because you asked a question of Dr. Field here, and I tend to carry all my knowledge in the suitcase.

Perhaps I could start out by answering in a somewhat different version the question you asked about Dr. Penzias' discovery of the background radiation.

What you see here is the picture of a somewhat peculiar clock which represents the action of the timing of the universe. And it's not the kind of clock you buy in the store; it's a clock which starts out going extremely fast and slows down by a factor of 10 every time it passes a tick mark. And in fact, it goes so quickly that the first second of time ends you up over here.

By the time the pointer points over here, the universe is 10 seconds old. By the time it points over here, it's 100 seconds old, and so on.

It has to become several hundred thousand years old before it becomes transparent. At that point you can now look back in time and see the last time that the universe was still opaque. That's when it was shining, and after that it becomes transparent.

Mr. **Nelson**. Does it become transparent because it's expanded so much?

Mr. **Harwit**. No, it's become transparent because of two things. One, because it gets sufficiently cool so that electrons and protons combine to form the atoms we see now, and atoms allow light to come through very quickly where the electrons don't—they scatter the radiation.

So all of a sudden, here it is crystal clear, and you look through and there you see back in time. And that's where what George Field was saying becomes relevant; namely, that this is all expanding away from us and that's why you're seeing it already shifted.

Now, if you sort of wanted to get a sense of scale, actually, almost everything we are talking about here today occurred in this very last bit here. In fact, if you expanded the tick mark between 59 seconds and 60 seconds, you could blow it up and you would get the entire history of the solar system, the Earth formed 4.5 billion years ago, and aerobic life originates, then where the 60 starts, you get cells with nuclei coming in about 3 billion years into the existence of the Earth.

Then multicellular organisms and plants about after 3.5 billion years. Man comes in here at the very last instant. It's a time which can't be distinguished from the center of this tick mark. I have way exaggerated it.

Well, one more sidelight on that question here, and that is that if you now want to look back in time toward this barrier, where the universe gets opaque you find,in fact, that this is so smooth that you cannot have started forming galaxies or stars very well before this layer here originated. If galaxies had started forming, you'd see patchiness in there.

But when you look, everywhere you look it's always smooth, and so you know that galaxies, stars, all the things that we see in the universe in our telescopes now have to have been formed more recently than that, more recently than about 10 billion years ago.

In order to study these galaxies as they might be formed, you need things like SIRTF, the infrared telescope that we are planning, or the Hubble space telescope. And to see the earliest quasars, which might have formed either before or after galaxies—and we really don't know yet—you need x-ray observations with AXAF and gamma ray observations.

Also, the microwave background radiation can be seen with radiotelescopes from the ground, but the quasars would also have to be studied, and preferably from in space.

Well, this is really more of a sideline. I am going to take up your offer of putting the written work that I submitted into the record and speak freely instead.

Now, what to me is significant about what I would like to say here is that we in astronomy do think about what it is we come in here and ask for, and what I would like to do here is to show the rationale that one follows in trying to understand what it is we should ask for, what it is that it might cost, and perhaps more important from a national point of view, is there any end in sight to the amount that we are going to be asking for?

What I am hoping to do here today is to sort of provide a sense of proportion about what this enterprise costs and why it costs that and what we want to do with all these different telescopes we come in here and ask you for and that you have so kindly provided.

Now, it goes without saying that there has always been a strong interest in the sky and the heavens and the way they work from Stonehenge in neolithic times through the first instruments that Galileo used to point at the heavens to discover the Jupiter satellites, through the Hale Observatory, and now the Hubble space telescope that we are talking about.

Mr. **Nelson**. Is that Stonehenge in England?

Mr. **Harwit**. Stonehenge is in England, that's right.

Mr. **Nelson**. And what was its relationship to astronomy?

Mr. **Harwit**. It's relationship was that it was used to determine how far north the sun was going to be coming in summer so that one could tell perhaps when to plant seed, and one could keep time by looking at the sun moving north and then receding to the south again in the fall and through the winter.

Now, this interest in astronomy is, of course, being pursued today, and you see signs of it everywhere. You see signs of it in the tickets that are bought in planetaria, down the street here at the National Air and Space Museum, where 15 million people a year have been coming to look at space exhibits. Another 350,000 a year are willing to spend money on the tickets to planetarium shows. And there are thousands of such planetaria in the United States, many in high schools where they enable one to educate children and tell them about the universe and about the world we live in.

Mr. NELSON. As further evidence thereof, the most frequented museum in the world is right across the street.

Mr. HARWIT. That's right. And I had the pleasure of working there for about half a year there one time, upstairs, of course. But I would sneak down and look at all these exhibits. [Laughter.]

Now, this telescope—actually, there are two of them here that Galileo had built—those marked a tremendous change in the way that astronomy was going to be done forever thereafter, at least up to the present time. One started building more and more sophisticated instrumentation, and as a result of that, one discovered new phenomena that had previously been completely unknown in the universe.

In the last few years alone, we have discovered pulsars in 1968, x-ray galaxies in 1966, the gamma ray background in 1968, superluminal sources—these are sources that fly out of a galaxy at almost the speed of light—just a few years later.

There has been this accelerating accumulation of discoveries as a function of time dating back to 1609 when Galileo first picked up a telescope and looked at the sky, right up to 1979, the period at which I stopped looking at this accumulation that's shown here.

Now, during this time we have built instruments of exceeding sensitivity at gamma ray wavelengths, x-rays, the ultraviolet, visible, in the infrared and at radio wavelengths. And if you look at the progression of sensitivities, you see in gray here some of the earlier instruments that have been built, IRAS, for example, which flew a couple of years ago, the HEAO-2, or Einstein satellite, which George Field mentioned, and you see that we're talking about factors of a thousand here between these horizontal marks.

You see that SIRTF, for example, is going to be a factor of a thousand times more sensitive than IRAS. AXAF is going to be a hundred times, close to a thousand times more sensitive than HEAO , and so forth. We are investing a great deal of effort in that direction.

We are also investing a great deal of effort, as George Field mentioned, in the direction of getting higher-angular resolution. And by that I mean the following: that you look at a map of the sky, and you see a radio galaxy here, out of which a blob is receding at the speed of light, or very close to it. And there is a powerful source here, you take this frame and you blow it up, and you see that there is a little jet from which this blob is rushing out. You blow it up again by another factor of 10, and you look at the detail. You blow it up another factor of 10, and so on. By the time you have blown all these up, you're down to a millisecond of arc, which is the size of a dime in California. If you go up on the roof, that's what it would look like.

Now, angular resolution is one thing, but you also want to know color discrimination. If you have spectral resolution or ability to discriminate colors, by looking at the intensity of different lights at different wavelengths, as obtained here from the IUE ultraviolet satellite, of a quasar at an enormous distance, you find that you can tell not only the chemical constituents—here is carbon, very strongly ionized, that is, with many electrons pulled out of it, and hydrogen—you find that you cannot only tell the chemical composition but also the temperature, the density, and the speed at which these recede. All of that information is there, and we are able to analyze for it.

And so when one talks about the instrumentation that we will be needing for the future, it's fairly easy to tell you precisely what will be needed because it turns out there are only six parameters that characterize any instrument. Once you give me those, I can build that instrument, and there is no other description that is required.

We need to know the wavelengths or energies to which the instrument responds, its sensitivity, its angular or picture resolution, its spectral or color resolution, its capacity to discriminate polarization as polarized glasses will in the visible, and its time resolution—its ability, for example, to detect these millisecond pulses that come from pulsars.

Before we had the capability of millisecond resolution at radio wavelengths, pulsars remained unknown. One sensed the radiation, but it looked just like a blob in the sky with nothing peculiar to it. Once you speeded up your time resolution, you were able to see these little blips and you were able to see that there was something enormously interesting going on there.

Now, if one looks at the history of the evolution of instrumentation—and here I am talking about angular resolution—you see the following. Again you see a display of the different wavelengths going from gamma ray to x-ray visible, infrared to longer wavelengths at radio, and the radio regime. And you see plotted here the angular resolution of which we have become capable.

Mr. **Nelson**. If you can focus that?

Mr. **Harwit**. Yes; the machine, unfortunately, doesn't seem to be terribly good at focusing. Perhaps if I moved it back a little, it might at least be bigger.

[Pause.]

Mr. **Harwit**. Is that better?

Mr. **Nelson**. Yes. Thank you.

Mr. **Harwit**. You see that you have angular resolution up to something like 10,000 million, or 10 billion, coming in here, which we have already achieved at optical wavelengths and at radio wavelengths. At radio wavelengths we're doing about as well as one can do with telescopes that are distributed around the Earth, and we now have to go out into space to do better than that.

Now, if you notice, I plotted here the capabilities we had in 1939 dark green, 1959 an intermediate green color, and 1979 a lighter shading of green. The areas that are white, we still have no capabilities in.

Now, what you see here is an expanding wave of capabilities, instrumental capacity to make observations. And in the wake of that, you see crystallizing out these colored spots, which are the discoveries. You see, for example, in 1963 the discovery—1960 to 1963—the discovery of quasars which in 1959 we had no instrumentation capable of detecting.

Again, over here you see the discovery of x-ray stars and x-ray galaxies, respectively, 1962 and 1966, which again 5 years, 8 years earlier, we had no way of discovering. We didn't have the equipment to do that.

Now, NASA has taken this to heart, and so what they have done has been to design the next generation of satellites in such a way that, for example, GRO has angular resolution capabilities in this white area here, way beyond what we have been able to achieve today. The same thing with AXAF.

It's a little harder with the Hubble space telescope. It will have capabilities in time resolution unparalleled. It will also have close to the best angular resolution, but not the best achieved so far. And again, SIRTF particularly will have spectral resolution capabilities that we have not had before.

Now, the important thing to recognize is that there are severe borders to this, one on the right-hand side, one on the left-hand side, and one on top. These borders tell you the natural limitations that the universe itself places on what we can observe and cannot. We cannot observe beyond here because the gases in our galaxy absorb all this radiation. We cannot observe over here because again the radiation in the universe prevents light from passing through when it is energetic. So we are only going to be coming to you for more funds for telescopes until we have filled this space up.

And you can see that we've done quite well in the last few years, so that hopefully we will not be coming back 100 years from now asking for more money because we will already have the telescopes.

Now, I mentioned the rapidity with which the discoveries I just enumerated were made. To give you a better idea of that, this viewgraph shows the number of discoveries that are made within 5 years of the availability of the instrumentation which was crucial to the discovery being made.

You can see that between 1964 and 1979, the bulk of these discoveries were made almost at once. There were no discoveries made with instrumentation which had been available 25 or more years earlier.

This is not all that unusual. Even in the time between Galileo's use of the telescope in astronomy, and 1964, a period of 350 years, you find that many discoveries are made in less than 5 years. Galileo's himself was made within a year of the availability of the telescope. There are relatively few discoveries made with instruments that were older than 50 years.

So the instrumentation is an absolutely crucial element of the discovery. And again, if you look at spectral resolution—I don't want to dwell on this—but here, increasing spectral resolution means increasing discrimination to colors, again across the wavelength regime. And you can see again that NASA is planning to build GRO with extremely high spectral resolution compared to previous ones, AXAF with even more, and so on, throughout a large variety of different instruments. They all are expanding this front into a regime which we have not had the possibility of making observations in.

If you notice this sharply increasing curve that I showed of discoveries is expected then to continue just by virtue of this alone.

Now, you might say, "Well, why do you need all these different telescopes," and the best way perhaps to explain that is to say that when you look at our own galaxy, you see entirely different phenomena when you look with different telescopes. GRO would, in looking at our galaxy, occasionally see a burst of gamma radiation, once every few weeks, lasting only ten seconds, from a direction unpredictable. We don't understand these properly, and we have models for them but we are not sure that they are correct.

AXAF sees pulsing x-ray binary stars. Invisible light received, the ordinary stars in our own galaxy. At infrared wavelengths we see dust which has been heated by starlight and dust where new stars are being formed. These are the exciting regions that we are looking at these days to see how stars might have been able to form in the galaxy in recent times.

And when you use the radio telescope, you can see the hydrogen gas between the stars and the distribution that they follow.

These are extremely important to us, because if we want to explain what we see, we have to be able to distinguish different physical processes that are going on in order to come up with explanations that are going to satisfy us.

The questions that we keep posing and that we hope to answer with the great telescopes, the family of great observatories that we mentioned, are the following: We want to know how did the universe begin? What are the primary physical laws that govern its evolution? How did galaxies form? How did stars form? How did planetary systems form and life begin?

These are the fundamental questions that have been asked for a long time. We are coming much closer—George Field was just talking about the planetary system that is being observed. So we started seeing one, we can now start studying it and so forth.

But as we enter the next generation of telescopes, we will have effectively a permanent presence in space. What the space station which the President has expressed so much interest in will be able to do for us is to provide us with refurbishment of the telescope. Instead of sending up telescopes for just 1 or 2 years as we have been able to do in the past, we will now have permanent observatories in space, and these permanent observatories are going to be quite expensive.

Now, let me explain this because this is I think where one puts the cards on the table, and I thought you might be interested in this. In the past, we have had the following sort of a picture. We always had some firm commitments—if you take the budget in any given year and you look into the future, you have a certain amount of money, and the amount of uncommitted funding this year is always very little. But you know that you are going to be phasing out certain projects, and so you see that your firm commitments come down rather rapidly as you look 3, 4, 5 years into the future.

Some of these, you anticipate continuing, and so you don't have all of this space here available for new ventures. What you have available for new starts and interesting new projects which flexibility demands are the light pink portions up here.

Now, you can see that if you have permanent observatories, you are going to be moving these lines up along the directions that the arrows indicate, and you are going to squeeze out any new ventures that you might ultimately be able to think of, particularly not only will you have these permanent observatories in space, but in order to make use of the data flow that is going to be coming in, you will also require massive ground-based facilities for computing, for storing the information, for rearranging it to see the overall global picture, which we call understanding.

Here, you see a facility which is the IUE—the ultraviolet satellite—ground station. It's going to be dwarfed by the stations that we will be requiring. So finally what we will be needing is a budget which slowly grows but in a way that perhaps would make sense—that is an astronomy budget which would be pegged to the gross national product, so that the country would be investing a constant fraction of its investment in astronomical investigation.

So we're not asking for more in a fractional sense. As the country gets wealthier, we would get a constant fraction. And here I have drawn a line which represents a GNP growing at a rate of 2.5 percent per year. This would allow us to have some commitments that are almost flat. Occasionally, we would add a new observatory. Occasionally, we would retire an observatory. By and large, though, we would be going up because we would have four, five, or six observatories in space.

I mentioned before that it's the synergism between these which really provides the understanding, and this is why we would like to have all of them in space at once rather than being phased out. You would have a small wedge here which would be available to use for the whole time for looking ahead to the future and planning astronomy that way.

Finally, to summarize and to add one more element to this, what we would like to have is the expeditious launch of the family of great observatories, which we have mentioned. This should include full support from the space station and the astronauts for refurbishing, for planning of the platforms that will be required to hold the telescopes extremely steady for high-angular resolution; funding pegged to the growing gross national product.

Then an item that I haven't talked about but I know that you are going over to the Soviet Union this evening. I am going over to Holland this evening because I am a member of an international team of scientists on the infrared space observatory there.

International collaboration can help a great deal in making monetary sense, fiscal sense; that is, we have increasingly had Europeans come up with highly sophisticated gamma ray observatories. For example, the COS-B observatory has been the highest-sensitivity one that we have had in the past. They are now building an x-ray observatory which will be the best one while it's flying. It won't be as good as AXAF, but it will be an outstanding instrument. The Japanese have also put up x-ray observatories that have been extremely valuable scientifically.

It makes sense to try to cull the best out of each nation and plan things in such a way that even though supported by individual nations, these observatories would be dovetailed in a systematic way so that we cover all of the areas that we want to cover to make sure that there are no gaps. It will require collaboration both between agencies and between individual scientists, and I think we should try to pursue that with as much vigor as we can.

Thank you very much.

[The prepared statement of Mr. Harwit follows:]

THE PROMISE OF ASTROPHYSICAL OBSERVATIONS CONDUCTED FROM SPACE

Martin Harwit
Astronomy Department, Cornell University

Overview

The great successes of astrophysics in recent decades have exhibited two clear trends:

1. Major discoveries have swiftly followed the introduction into astronomy of novel, powerful observational instrumentation.

2. The growth in our understanding of cosmic processes has crucially depended on full scrutiny, in all accessible energy regimes, of radiation arriving not only from within the solar system and the Milky Way, but also from the most distant portions of the universe our telescopes can reach.

When we look to the future, these trends unwaveringly point to the need for a family of long-lived observatories in space coupled to massive data handling facilities on the ground.

The promise of such a program is outstanding; its projected costs appear compatible with currently enunciated national priorities.

* * * * *

The Promise of Astrophysical Observations Conducted From Space:

Historical evidence abounds for a continuing interest in the workings of our universe exhibited by all major cultures throughout recorded time. That interest is still powerfully reflected in the United States today. The National Air and Space Museum, the country's principal museum dedicated to space exhibits has attracted up to 15 million visitors in recent years; more than 350,000 of these pay admission to attend planetarium shows, and attendance continues to grow. A thousand other planetaria exist around the country, most of them in high schools where they can play an especially significant educational role.

Modern astronomy dates its beginnings to 1609 when Galileo first looked at the heavens with a spyglass -- an instrument that had been invented only the year before. That one invention drastically changed both the direction and the pace at which astronomy was going to be conducted, thereafter: Increasingly, astronomers came to utilize advanced observing techniques, not only at visible wavelengths but also in other parts of the spectrum. The most striking recent advances have depended on the adoption of instruments that have permitted the detection of radio waves, infrared emission (heat), and X-rays as well as the even more energetic gamma-rays. These have made possible the discovery of quasars, pulsars, neutron stars, whole galaxies which emit the bulk of their radiation as X-rays, other galaxies that emit most powerfully at infrared wavelengths, and ubiquitous radio background radiation reaching us from the most distant portions of the Universe ever observed -- attesting to an intensely hot explosive origin of the Cosmos.

The importance of advanced instrumentation in these discoveries is readily documented, since instruments can be quantifiably classified according to just six properties: The wavelength or energy to which they respond; the sensitivity with which observations can be carried out; the angular resolution or image definition they can provide; their spectral resolution or ability to discriminate different colors, wavelengths or energies; their capacity to discriminate the polarization of radiation; and their capability to follow time variations in the signals observed. Together these six measures provide a full account of an instrument's ability to respond to a particular carrier of information reaching us from the Universe -- whether it be a stream of light quanta, an influx of atoms or electrons, or an occasional hit by a dust grain of cosmic origin.

When we now examine the history of technological advances in angular resolution for all wavelengths of radiation -- gamma-rays, X-rays, ultraviolet and visible light, infrared and radio waves -- we see an expanding observational capability as we go from the year 1939 to 1959 and on to 1979, bringing us close to the present. In the wake of this expanding front of technical competence we see new discoveries crystalizing with amazing rapidity. Quasars, discovered at radio wavelengths, first became recognized around 1963 through the use of instrumentation unavailable just four years earlier, in 1959. X-ray stars and galaxies, respectively discovered in 1962 and 1966, also were first detected with instrumentation lacking in 1959. A similar history marks any of the other instrumental parameters mentioned above: Instrumental prowess in spectral resolution or sensitivity has consistently led to new discoveries and ultimately to advances in understanding. We frequently find a discovery occurring within five years of the date at which instrumentation essential to the discovery first became available. In the twenty-five years between 1954 and 1979, not a single major discovery was made for which the requisite instrumentation would have been available a quarter of a century earlier. Novel, more powerful instruments were absolutely crucial to the realization of these discoveries. Moreover, the modern era is not alone in this requirement. We find almost the same immediate relationship between instrumentation and discovery for the entire era beginning with the invention of the spyglass and leading up to 1954.

Recognizing the importance of novel instrumental capabilities, NASA has planned a family of observatories, to be launched in the next few years, in which instrumental advances over and above those exhibited by earlier facilities are clearly apparent: Under this Great Observatories in Space program the agency expects to launch the Hubble Space Telescope (HST), the Gamma-Ray Observatory (GRO), the Advanced X-ray Astrophysics Facility (AXAF) and the Space Infrared Telescope Facility (SIRTF). These long-lived, orbiting observatories should be active throughout the 1990s. The extensive wavelength coverage they will provide spans most of the spectrum accessible solely from space -- all the way from the gamma-ray domain, through X-ray, ultraviolet and infrared wavelengths, to that part of the spectrum where ground-based radio observations again become possible. Their sensitivity will be far higher than any available to date. Their spectral resolving power will advance instrumental capabilities into domains unreachable before. With such powerful technological innovations, this family of Great Observatories in Space should continue to uncover new phenomena whose significance will match the greatest discoveries of past decades.

Experience shows that we have been most successful in explaining complex cosmic phenomena when we have had at our disposal observations covering this entire wavelength range. Only in that way can a variety of different processes potentially underlying a given observed phenomenon be distinguished from each other. Each process typically makes itself known at quite different wavelengths: When we observe the Milky Way at optical wavelengths, we see a steady stream of light from stars. Infrared telescopes sense the heat emitted by interstellar dust, and permit us to trace the distribution of dust clouds throughout our galaxy. X-ray telescopes, in contrast, can register X-ray pulses emitted by compact binary stars, while radio telescopes present us with a map showing the distribution of hydrogen clouds pervading the interstellar spaces. Finally, a gamma-ray telescope scanning the Milky Way would observe occasional powerful bursts of gamma-rays from unpredictable directions at intervals of a few weeks. A burst would last only a few seconds and then cease. We hardly understand the cause of these eruptions.

Ultimately the aim of astrophysics is to clarify:

- The origin of the Universe

- Its governing laws of physics
- The birth of stars, planets and life

These are the goals as we approach a new era that can be realized with the inception of the Space Station, an era of Great Observatories for Space Astrophysics, an era that can offer astronomy a permanent presence in space. The United States can continue to play a leading role in this coming era if our space program is vigorously pursued.

Such a program has four principal requirements:

Requirement 1.

The family of Great Observatories in Space needs to be active in space throughout the 1990s. HST, the first of these, is currently scheduled for launch late in 1986. GRO, the next observatory to be launched, is well on its way as an approved mission; but AXAF and SIRTF still are waiting in the wings and urgently require, respectively, a new start and early approval for phase-B studies. These steps are essential for effective collaboration to go forward with Space Station engineers, so that we may successfully design co-orbiting platforms in space, engineered to match the high stability- and servicing-requirements these permanent observatories will demand. Accordingly we recommend:

Recommentation 1.

- Expeditious launch of the four Great Observatories which constitute not only NASA's but also the National Academy of Science's highest priorities for space astrophysics in the 1980s.

Requirement 2.

In the Space Station era astronomers will be offered unique new opportunities for erecting large interferometers in space, by means of which we will be able to define the structural features of astronomical bodies with resolving powers totally unknown today. We will also be able to build massive cosmic ray detectors capable of probing the nature of particles cosmically accelerated to enormous velocities and energies. For the satisfaction both of this requirement and of the first requirement outlined above, we recommend:

Recommendation 2:

- Full support from Space Station, including the capability for servicing observatories now in advanced stages of planning or facilities about to be launched as well as a capacity for erecting complex structures in space when these are too large to be launched as a single unit. Space Station astronauts would figure centrally in these tasks.

Requirement 3:

Like the 200-inch telescope on top of Mt. Palomar, the Great Observatories will have useful lives far beyond anything built in space, to date. This will have significant consequences on funding requirements: In the past, it was possible to retire instruments that had outlived their usefulness, and this made room in our budgets to fund new ventures. In contrast, the 1990s will bring us space observatories that can be serviced and refurbished to provide decade-long observations, allowing the study of gradually evolving processes while also permitting powerful investigations of new phenomena which our current discovery rate indicates we must expect to keep uncovering: In this era of long-lived observatories the cost of mission operations and data analysis by massive ground-based facilities will place a constant burden on the astrophysics budget. New instrumental capabilities, new ways of examining the Universe, will only continue to be possible if the astrophysics budget is permitted to grow. A powerful program can be maintained if we are able to fund astrophysics at a constant fraction of the Gross National Product. We urge:

Recommendation 3:

- o Funding of the Astrophysics program starting at its current, 1985, level and growing at an annual rate pegged to the growth in the Gross National Product -- an annual growth rate in the range of 2.5 percent.

Requirement 4:

In recent years, scientists overseas, particularly in Western Europe and in Japan, have been competing with us in astronomical ventures of the highest caliber. The European COS-B mission obtained some of the finest gamma-ray data on the structure of our galaxy; the ROSAT X-ray satellite to be launched in the next few years will be the most powerful X-ray observatory in orbit during its limited lifetime; and the Infrared Space Observatory, ISO, will be an important tool for studying the formation of stars in the Milky Way. In Japan X-ray astronomy from space has also made striking contributions. We recommend:

Recommendation 4:

- o A cost-effective program that maximizes scientific return through open discussion and international cooperation among scientists as well as agencies. Increasingly, we will need to coordinate missions which, though funded and launched by individual nations, require dovetailed joining on all sides. Increasingly, also, we will need to emphasize cooperative efforts where resources can most effectively be pooled.

Given the cost of operations in space, it only makes sense to carefully coordinate our scientific and engineering endeavors, to proceed expeditiously at a steady, adequately funded pace, and to pool resources internationally to effect a powerful investigation into the nature of the Universe we all share.

Mr. NELSON. I want to follow up on that with some specific recommendations that you might have for us in our meetings with the Soviet Academy of Sciences.

Dr. Hoffman.

STATEMENT OF DR. JEFFREY A. HOFFMAN, ASTRONAUT OFFICE, LYNDON B. JOHNSON SPACE CENTER, NASA, HOUSTON, TX

Dr. HOFFMAN. Thank you, Mr. Nelson. It's a great pleasure to be here today. I guess you realize I was invited here, in large part, because of my staus as a NASA astronaut. I will be talking on my own behalf and not on the behalf of the Agency.

I would like to say something to carry on this point that both Drs. Field and Harwit made about the popular interest in astronomy. I have always looked on astronomy as a cultural activity. You know, physicists, biologists, and chemists can often point to the practical engineering applications of their research. I think that is less the case in astronomy than many other subjects, and yet it's continually amazing the great level of popular interest in astronomy as a science.

Only this past Monday, I was up in New York taking part in the celebration of the 50th anniversary of the Hayden Planetarium. They've had about 25 million visitors there since the planetarium opened. It's certainly the place where I cut my teeth as a young child and got interested in astronomy.

They did have several hundred school children among the invited guests, and I would like to say that I think that when we look at some of the benefits that we get out of space research and the space program in general, aside from all the spinoffs, all the things which we can at least try to quantify, one thing that is absolutely impossible to put a number on is the inspiration that young people all around the country get.

You know, we talk about the crisis in education, the difficulties in getting children to study the hard subjects, and yet the need that our country has for more scientists and engineers. How many students, like perhaps Scot here, originally got their inspiration to put in that little extra bit of effort and work hard, study the hard subjects, because of the fascination with the space program or with astronomy. We can't put a dollar figure on it, but I am convinced that that must be one of the most important things that we're getting from the space program.

I am going to talk about astronomy done from space, partly from the point of view of a practitioner of x-ray astronomy. I did my graduate work and professional astronomy in the field of space science, carrying out observations from balloons, rockets, and satellites above the Earth's atmosphere.

It's hard to tell whether I got involved here because I basically liked the idea of using rockets and satellites. I had this dream even back then—or probably both—I always had felt that that was one of those fascinating areas of astronomy. We are at the point where every time you do devise a new instrument, as Dr. Harwit pointed out so well. you are almost guaranteed of making exciting new discoveries, and this has certainly been the case throughout my career.

But astronomy is very old science, and on several occasions it has gone through the phases that we recognized as being characteristic of the development of scientific disciplines. Typically, when an area of science is developed, you have to start out by basically just making observations, realizing that there are new things to be seen. Once you've made observations for a certain amount of time, then you gradually start to perceive some regularities and patterns. Then, only once you get past that basic stage are you able to apply the tools of mathematical analysis to turn a field into what we consider hard science. Certainly, classical astronomy went through this stage with the plotting of the positions of the stars and planets, culminating in the Newtonian revolution when Newton's theory sort of systematized all known astronomical knowledge at the time.

Astronomy has gone through this phase on several occasions. In fact, every time we develop new instruments which open new areas of the universe either in terms of a new wavelength to be studied or a new time resolution or just a new power which allows us to see farther away and see objects which we couldn't see before, we go through this same phase.

At this point in most areas of astronomy, astronomers have reached the point where we really do rely on making our instruments bigger and more powerful in order to gain new knowledge, and this, by and large, means that the great new discoveries of astronomy are going to be made by these great observatory instruments. These are not manned instruments. They will be made by unmanned observatories. And I think that certainly that's the great thrust in this field.

So I am often asked what role does man play in the development of astronomy since astronomers basically do use telescopes and telescopes are something which we have learned even on the Earth to use remotely. In fact, I spent 3 years of my professional life working in England. The skies in England, as you know, are not very clear, and the British astronomers have always enjoyed going all over the world to operate their telescopes, including some of my good friends who enjoy going out to the observatories in Hawaii, a welcome change from the British climate.

There, some of them are appalled now at the new developments in satellite technology which are allowing data to be transmmitted directly back to their home institutions in Britain so that they can observe using a telescope on the mountaintops in Hawaii without ever leaving the dismal climate of their home country, and there is some serious question as to whether that's really a technical advance or not.

But certainly, it's that sort of technology which allows astronomers on the ground to make use of telescopes in orbit. And I don't want this to seem, you know, heretical from an astronaut, and especially one who is planning to fly next March on what is going to be actually an observatory flight where we will be carrying telescopes onboard the space shuttle to make astronomical observations.

As I say, I think these large telescope observatories are the most important thing in astronomy, and I think it's essential that they receive the country's support if the United States is going to maintain its position as a world leader in astronomical science.

I do think there is a lot of valuable work which can be done using the space shuttle as an observing tool, and I would like to talk about that for a little while.

First of all, of course, I think the most important thing that the shuttle has done for the development of space technology is in what it has allowed us to do for increasing the technical complexity of satellites. This was referred to briefly. I think the space telescope, just as an example, would be unthinkable without the space shuttle, despite the fact that we don't operate the space telescope from the space shuttle.

I mean, the investment in the space shuttle as a project I think would be almost impossible to contemplate with the idea that this might just be something that was launched by an expendable vehicle and if it malfunctioned we could never get at it to fix it, and the fact that we would be stuck with an observatory whose instruments we could never change. It would have been as though we had built Mount Palomar back in 1948, the great Hale 200-inch telescope, and then continued to use the same camera technology that was around in 1948, which is totally obsolete at this point.

So I think that is probably the number one contribution that the space shuttle has made and will be making to astronomy in the future is the level of sophistication of astronomical telescopes that it permits.

I think there are reasons for using the shuttle in a direct observing mode, and I would like to talk a little bit about that. Astronomy cannot be kept healthy as a discipline with only just a very small number of super-large observatories.

I think, using ground-based astronomy as an example, if all we had were the 200-inch Hale telescope at Mount Palomar and everybody had to use that instrument, first of all, you couldn't support that many astronomers. There just wouldn't be enough time to get on it.

Second of all, how would you develop the new instruments, test them out, test your ideas, prepare them for use on a telescope whose observing time is extremely valuable because everybody wants to get on it?

We need the opportunities to develop new instrumentation that will be used on space telescopes of the future. We need the opportunity to make mistakes, as the case may be. The shuttle offers us the opportunity to go up, try something out, see how well it works, fly it over and over again.

The astronomical payload that I will be flying with, the first flight will be made this March, it's called the Astro mission. It's a collection of three ultraviolet telescopes, ultraviolet radiation, of course, doesn't penetrate the Earth's atmosphere. That's why we have to go up into space to observe the ultraviolet. We have observed ultraviolet radiation from a few astronomical satellites. While they're all modest, they're very valuable nonetheless.

Mr. Nelson. What is the ultraviolet experiment that Hoot Gibson was just telling me that we'll be doing on Columbia in December?

Dr. **Hoffman**. I will be referring to that in a minute. It's called the Champ experiment, and I will get into that because I think it's a good example of another aspect of shuttle science. I am glad you brought that up.

The Hubble space telescope, of course, is sensitive to ultraviolet radiation, and we've chosen, I think, a very valuable collection of detectors which will be used in the space telescope.

But generations of astronomers in the future will be thinking of new things to do with the space telescope, and I think, in a sense, through testing out instruments like some of these instruments we're flying on Astro—just as one example, the ultraviolet polarimetry is a very new field, probably too risky. The technology isn't that well developed to include an instrument like that as part of the complement right now of the space telescope, we can try these things out, and we will be flying the Astro on numerous occasions, so we will be able on hopefully 8-month intervals to replan future missions on the basis of what we learn in the past.

That's sort of the way astronomers work on the ground, where you might have maybe one or two observing sessions every year and you go back to your office at your university you study what you've learned, and you replan, decide what you want to do to make your next observing session that much more valuable.

I think one of the other most important things that we can get out of doing experiments on the space shuttle is the opportunity that it gives us to train the new generation of space scientists. It certainly is extremely valuable from a scientific point of view to have graduate students work on data coming back from these large observatories. You can get tremendous scientific return from that, and it's a very exciting thing for a student to work on.

But who is going to build the next generation of astronomical instruments? How do we train a generation of astronomers in how to use space technology? I think without the opportunities to fly fairly frequent and modest experiments—and I think the shuttle is the way to fly them now—we lose this opportunity. This is the sort of thing that traditionally has been done using balloons and rockets.

But I think as technology in astronomical observations has improved, the ability to do really useful science from, certainly, rockets have diminished tremendously. I think there still is a fair amount of useful work that can be done from high-altitude balloons, but for astronomical purposes they are somewhat limited, certainly in most wavelengths. And the shuttle is, I think, the hope of the future.

Now, this is not something new I am saying. People have recognized it, and this has resulted in very valuable programs like the Getaway Special Program, the Spartan Program, the Hitchhiker Program. I have worked closely with people involved in those programs. They are excellent programs.

I think the problem is they have been so slow in getting started that we really have not developed the capabilities to replace the opportunities which traditionally astronomers have had using balloons and rockets. And things really have been difficult, I think, now for 10 years to achieve the level of small-scale experimentation which was done so readily when I was a graduate student.

I think that that is a gap in our capabilities at the moment. I hope that the Spartan Program will continue to accelerate. Unfortunately, when you look ahead at the number of flight opportunities which we actually have scheduled, it's very limited, and it has not been able to pick up the slack left by the diminishing capabilities of the rocket program.

Mr. NELSON. Let me interrupt you right here.

Dr. Field, you've been here throughout these hearings. One of the witnesses yesterday or the day before was minimizing the effect of the shuttle and maximizing the effect of sounding rockets. Do you remember hearing that?

Dr. FIELD. I believe that I remember comments to that effect; yes. Your point is, what, that you're trying to see whether I would agree with that? Is that the point?

Mr. NELSON. Yes. Dr. Hoffman is suggesting exactly the opposite.

Dr. FIELD. Yes, I understand.

Mr. NELSON. Although he is speaking specifically about astronomy, whereas the other scientist may have been speaking about other sciences.

Dr. FIELD. Yes. I think, however, I would agree with Dr. Hoffman that planned transition from sounding rockets to Spartan and other types of activities aboard the shuttle is crucially important to bring about. It has been delayed unnecessarily, in my opinion.

The point was that the rocket program has been phased down by NASA, and the corresponding program for small activity, smaller experiments, have not been made available yet on the shuttle. That's how it affects astronomy. I am not sure how it will affect other fields.

Dr. HOFFMAN. There are other fields such as atmospheric physics, in particular, where rockets have made and continue to make very valuable contributions. The problem from the astronomical point of view is that you're only up in a rocket, a sounding rocket, for a few minutes and most of the objects of interest now are sufficiently faint that you just have to look at them with very large instruments for a long period of time in order to make useful observations. And even if you can do one thing in 5 minutes, that's only one object out of, you know, millions that we might want to look at.

From that point of view, of course, even a shuttle flight is rather short, and it's for that reason that, given the choice, I think most astronomers would prefer to put their telescopes in a free-flying satellite where it can be up for years rather than maybe a week that you can get out of a shuttle flight.

But as I said, there are reasons, especially during the time when you're developing new instruments and also from the point of view of training new astronomers in these techniques, that shuttle experimentation is extremely valuable.

I want to talk to you a little bit about my own experience. I, of course, was up in space. I was overwhelmed by the experience. I am sure you will be. The point of view that you get is truly unique. There is a whole set of phenomena involving the Earth, the sky, the near-space environment where—well, when I was a graduate student, one of my favorite songs was Joni Mitchell's "Both Sides

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Now," and that's the way I felt when I went up there, you know, I've looked at the Earth and I've seen it from both sides now.

Really, the ability to get up into this new environment of space is truly unique, and I think that there are many phenomena in the near environment of the Earth where scientifically speaking, we are still in this initial stage of the development of the science, where just having trained observers with a keen scientific mind and some very basic tools like cameras and other fairly simple instruments can make some useful contributions in the identification of new phenomena which are worth studying in the future.

Just some examples are observations we've made from the shuttle of lightning, storm development, auroral phenomena, ocean currents where the shuttle has been responsible for the identification of new types of current phenomena that oceanographers were not aware of before this decade and yet in which significant quantities of the total energy involved in the oceans are carried in these currents.

Many of the phenomena that you can see up there, they're transitory, they're fairly rare. You can't necessarily design a satellite that's up there making permanent observations to see phenomena that you weren't aware of before you designed the satellite.

So that I think there is a role to be played by human observers in space using human intelligence to identify new phenomena that is worthy of future study.

I would like now, referring to your question before about this ultraviolet camera, just to give you sort of the history of one development of this type of technology, sort of where we started with a fairly simple observation and have gradually been developing the sophistication with which we can study. And I think it's a good example of the sort of thing that we can do from the shuttle and how it spills over into other aspects of space science.

It was on the third shuttle flight, STS-3, which carried a scientific payload in the cargo bay. They took some long-time exposures designed to photograph a particle beam which was being generated by one of the experiments that was carried.

It was found, when we looked at those photographs, not only did we see some glowing light coming from objects in the payload bay, but the skin of the shuttle was glowing, a kind of an orange glow. This was very surprising. It had not been seen before.

So, the next shuttle flight, STS-4, the crew was specifically asked to take pictures, and they came back both with color photos and also pictures taken with the shuttle's TV cameras, showing that, yes, indeed, the shuttle skin really did glow, and not only did it glow, but they were able, by analyzing pictures taken when the shuttle was pointed in different directions, to show that this glow was associated with the oncoming rush of particles, the residual atmosphere that's up there even in the orbit of the shuttle. Of course, you still have a very thin atmosphere which causes a certain residual drag on all satellites.

Well, when the astronomical community started to hear about this, a lot of people started putting facts together. We realized that there is some interaction going on between the residual atmosphere. Perhaps the oxygen atoms up there were interacting, creating this light.

People were asking,

> Well, you know, what happens when the Hubble Space Telescope is up there? Are we going to get this? Suppose these atoms come and they go on the inside of the space telescope and the inside of the space telescope starts to glow. This is going to have a very serious impact on these low-flight-level objects which the space telescope is supposed to see.

So there was a great interest. There was a conference at Goddard on this phenomena. They went back to some earlier satellite data from one of the atmospheric explorers, I think it was, and identified some similar types of glow which I think had not perhaps been fully appreciated before. And I have to say that one of the things, if I had to give a little, you know, pet peeve about the Shuttle Program is things often move very slow when you're involved in manned space flight, and if you want to get a new thing flown in the shuttle, there is often a lot of paperwork you have to go through and it can be months or years before you got something through.

But the system responded brilliantly in this case, and on STS-5 we got not only a regular camera but actually a new piece of electronic equipment, an image intensifier which magnified the light level by a factor of about 10,000. So that we were able to photograph this glow with much more sensitivity, and we also had a very primitive spectral measuring device to try to look at what the chemical constituent of this glow was. And I was the support group for the fifth flight, so I was directly associated in the design and implementation and the carrying out of this experiment.

Then on STS-8 we got together with the people interested in the glow, and also another phenomena which had been observed sort of from another direction which was tied in with this, that we found that some of the material that was being brought back from space, some of the kapton, the thermal insulation, gave distinct signs of being degraded. The shininess was gone. Parts of it were actually eaten away.

The thought is that perhaps this is the same interaction with the oxygen atoms coming in. So we actually took some layers of material and put little barberpole stripes as you were on the remote manipulator arm which was flying on STS-8, and photographed these materials with this same image intensifier system. We were able to look at the correlation between the different glows, the different intensity of glows of different types of material and the amount of degradation of these materials, including, by the way, the same black paint with which the inside of the space telescope is covered.

I should say, in case you're worried about this, the net conclusion, I think, of most people is that we've measured now the intensity of this light level, and since the intensity of this glow is very much dependent on the altitude because the atmosphere gets very much thinner very quickly as you go up, I think the belief now is that the space telescope is at a high enough altitude that we're probably safe from this sort of light contamination.

On STS-14—that was 14-DMAS in the interim, going over to our marvelous new numbering scheme, we added a somewhat more sophisticated spectrograph where we actually had a slit where we could get a very high resolution spectra to study the glow from the shuttle, and I think one of the interesting conclusions is that we believe now that atomic nitrogen plays an important role as well as atomic oxygen.

Then I came into the picture a little bit more directly because the next time this equipment was carried was on my own flight, and I had the idea that, gee, now we have this low-light-light system together with the spectrograph, I wonder what would happen if we didn't just take pictures of the shuttle but maybe we can take some pictures of the stars. And, particularly, of course, we're all thinking about the possibility of taking pictures of Halley's Comet. And we do have some of these more sophisticated telescopes that will be flying on the Astro Mission which will study Halley's Comet, but the shuttle is going to be up on numerous other occasions, and wouldn't it be great if we could photograph it and study it on some other flights?

So I did some astronomical test photography using this equipment. It functioned quite well. We were pleased at the level of sensitivity which we could achieve, and that resulted in this CHAMP project, the Comet Halley Active Monitoring Program, and the first time that this equipment will be flown to study Halley's Comet will be on the December flight, so you will get a chance to see firsthand how you can actually do some fairly simple astronomical observations from the shuttle itself.

Well, I went through this whole story just because I thought it's a good example of how the shuttle can work at its best; that is, when we come up with new ideas and we can use the fact that the shuttle is flying at fairly frequent intervals and we can turn this equipment around. It's really doing science like you'd like to be able to do it in a laboratory where you don't have to wait for years every time you want to do a new experiment.

Unfortunately, there haven't been nearly as many success stories like this as I would like to say. It often takes a long time to get things manifested on the shuttle. Now, hopefully, with the increased flight rate, that will improve.

I think it's important to remember that in addition to the great, grand projects of the astronomy of the future, we have to keep nourishing the roots of the science by making it possible to do fairly frequent space observations and to give people the chance to develop new instruments, to develop new skills.

Just in closing, I would like to make another point, that we all know that the financial problems the shuttle had and the difficulties in the development.

Having now seen how the shuttle is used as a research tool—and now I am not talking just about astronomy but about the many other areas of science—biological material science—had we been able to spend, you know, just a fraction, you know, maybe 5 percent more on the development of the shuttle, we could have increased its capabilities as a scientific research platform manifold—and I am talking about things like extending the duration of shuttle missions, of having more powerful computer systems, you know, basically small stuff compared to the total cost of the development—it would have made a big difference in the scientific return.

I just want to voice the fear that we may find ourselves getting into the same situation in the design of the space station. We know how critical the budget is. You know, when we build the space station, the bulk of the money is going to go for the core station that's giving us the capability of keeping people up there for a long time, and it's the little extra bit that we should be putting in that will make the difference in how useful the Space Station turns out to be for scientific and research purposes. And I hope that the committee will keep that in mind when the budgetary review process comes up.

I am glad to have been able to talk to you today.

[The prepared statement of Dr. Hoffman follows:]

NASA
National Aeronautics and
Space Administration

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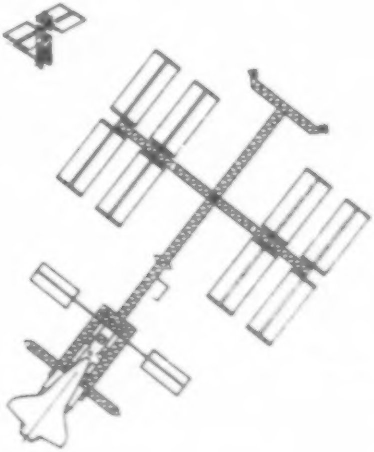
Presented by Witness

October 10, 1985

Subcommittee on Space Science and Applications
Committee on Science and Technology

House of Representatives



Statement by:

Jeffrey A. Hoffman
NASA Astronaut-Astrophysicist
Johnson Space Center

99th Congress

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Statement of

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Mr. Chairman, Members of the Committee, I am pleased to have the opportunity to address you today and to offer my personal views on the impact of the shuttle on Space Astronomy as an astronaut and astronomer. I have had the privilege of flying in the space shuttle on mission STS 51-D last April. While the mission received most of its preflight publicity from the presence onboard of a collection of toys and a U.S. Senator and most of its inflight publicity from our exploits with fly swatters, we did in fact also carry a small low-light-level camera system which I used to take some test astronomical photographs. This camera system will hopefully be used to photograph Halley's Comet next year. During the Astro-1 mission, which is one of the prime observing astronomy flights. I will be one of the mission specialists on that flight. With two astronomer astronauts and two astronomer payload specialists, the astronomers on that flight will actually outnumber the pilots! When I get back from the Astro mission, I will no doubt be in a better position than now to tell you what it is really like to do astronomy from the shuttle. For now, what I tell you is based on my knowledge of the shuttle program up to this time and of the research done in space astronomy using balloons, rockets and expendable-launched satellites over the last 20 years. My pre-astronaut professional career was devoted to high-energy astrophysics: x-ray and gamma ray observations made above the Earth's atmosphere. I therefore am familiar with the history of the development of space-based astronomy and can use my knowledge of the shuttle to speculate on how the shuttle can best be used for future astronomical research.

One of my first recollections about space astronomy, as a student in the mid 1960's, was hearing how the great new Orbiting Astronomical Observatory satellite was idly circling the earth, a victim of a simple battery malfunction. If only the shuttle had been around then, 20 years earlier, we could have fixed it on the spot! The capability of shuttle to launch and service satellites is important for astronomy, because satellites have become indispensable tools for astronomers in allowing the universe to be studied in all ranges of the electromagnetic spectrum, including radiation that never reaches the surface of the Earth. Later I will discuss making astronomical observations directly from the shuttle, but no matter how many shuttle flights we can put telescopes on, astronomers will always choose a long term satellite mission if the choice is there. Biologists and materials scientists have strong interests in hands-on experiments, and manned spacecraft are ideally suited to their microgravity research programs. Even on the Earth, astronomers work with their telescopes remotely, and we can certainly operate our space-based telescopes from the ground if we have sufficient communications facilities.

This may sound like a heretical statement from an astronaut, especially a crewmember of the first dedicated astronomy shuttle Flight. I do not want to soften it, however. I genuinely believe that the great new astronomical discoveries of the next few decades will be made by large, remotely operated spaceborne telescopes - the Hubble Space Telescope in visible and ultraviolet wavelengths, the Gamma Ray Observatory and the similarly scaled Space Infrared Telescope Facility (SIRTF) and the Advanced X-Ray Astrophysics Facility (AXAF). I also think that without the shuttle these projects would be unthinkable. We could not risk launching a billion-dollar project like the Space Telescope without the knowledge that shuttle crews can repair or retrieve it in case of a malfunction. Also, periodic visits by shuttle crews to these satellites will allow observing instruments to be updated and replaced as detector technology improves, much as is done for ground-based telescopes. Without the shuttle, telescopes such as International Ultraviolet Explorer (IUE) and Infrared Astronomical Satellite (IRAS) would probably have been the limits of what our technology would allow. I believe the most valuable contribution the shuttle will make to astronomy is the increase in sixe and complexity of astronomical satellites it allows us to operate as national orbiting observatories.

What about astronomical research from the shuttle itself? Under what circumstances is it preferable to fly an astronomy payload on the shuttle rather than on a free-flying satellite? The reasons are similar to the reasons for flying balloon and rocket payloads over the past 20 years, even after satellites moved into the center stage of new astronomical discoveries. These reasons are threefold: to allow the development and testing of new equipment, some of which will eventually fly on satellites; to provide continuing research opportunities for research groups without current satellite projects; and, to train new generations of astronomers in all phases of space technology.

I can illustrate the first reason with the Astro payload I am flying with in March 1986. The Wisconsin University Photometric Polarimetric Experiment (WUPPE), one of the three main Astrotelescopes, will break new ground by measuring the polarization of ultraviolet light from astronomical sources. Polarization measurements in visible and radio observations have been extremely useful in studying magnetic fields, interstellar dust and many other elusive phenomena. A prototype of this instrument flew once on a rocket, but both the technology and potential scientific utility of ultraviolet polarization studies are too unproven for such an instrument to be considered for a major astronomical observatory like the Hubble Space Telescope. The risk would be too great and the potential reward unproven. Without any other satellite opportunities, should we simply forego these observations? A single rocket flight simply does not provide enough time to observe any but the brightest objects in the sky. By flying this instrumentation on the shuttle, this new technology can be tested and hopefully improved. At the same time, valuable astronomical knowledge will result. The current manifest of these flights at 8-month intervals allows each flight to make use of the results of the previous flight, an efficient way to do science. If ultraviolet polarimetry as done on the Astro missions proves its worth, perhaps someday a derivitive experiment will be placed in the focal plane of the Space Telescope.

To illustrate the second reason - providing continuing research opportunities between major satellite projects - I refer to the present unhappy state of x-ray astronomy in this country which perhaps could have been prevented if more shuttle flight opportunities were available. I have a strong personal interest in this field, because it is where I did my own research. Celestial x-ray astronomy started in the early 1960's with the discovery of a few strong x-ray sources by rocket-borne instruments. Nuclear physicists and classical astronomers joined forces to refine detector technology and astrophysical theory and build what became one of the most dynamic areas of astronomy. Numerous x-ray satellites opened up the field tremendously. Pulsation binary stars, glowing intergalactic matter, x-ray bursts, active galaxies, black holes-all these exciting phenomena have been studied by x-ray astronomers. For 20 years, the focus of this research was in this country. Researchers came from all over the world to share our data. The culmination of the development of x-ray astronomy in the U.S. was surely the Einstein High Energy Astronomical Observatory. This observatory's power, compared to that of the first x-ray satellite, Uhuru (which was a superb devvelopment at the time), was like the power of the Mt. Palomar observatory compared to the telescopes of Galileo. With the reentry of Einstein into the Earth's atmosphere, active x-ray astronomy in this country came to an unfortunate halt. The data from Einstein are still being analyzed and will provide interesting science for years to come; but when these data lead astronomers to pose new questions, they have nowhere to go to answer them except to new satellites built by the Europeans and Japanese. The focus of x-ray astronomy has moved abroad. We have wonderful hopes for the Advanced X-ray Astrophysics Facility (AXAF) a super-Einstein observatory of the 1990's. But many research groups active in x-ray astronomy over the last two decades have been gradually withering away through lack of observational opportunities There are a few x-ray astronomy rocket flights every year, and the first shuttle Spartan payload last June carried an x-ray astronomy payload. However, the number of Spartan payloads is currently quite limited and for at least a few years cannot take up the slack of what looks to be a greater than 10 year gap between successive American x-ray astronomy satellites. American researchers now collaborate with European and Japanese groups to gain access to high-quality data. This state of affairs could be remedied by an increase in shuttle flight opportunities, which would give new vitality to research groups. That is at least what I would wish for the future if I had the power to influence events.

To illustrate the third reason for doing astronomical observations from the shuttle-the training of new space astronomers - let me relate some of my own training. For my graduate thesis project, I built a gamma ray telescope which flew four times on balloons. To say I got my hands dirty is an understatement. I learned how to tap screw holes, I learned about vacuum systems, I built digital and analog electronic circuits, I wrote computer programs to simulate the response of the instrument and predict its sensitivity. What fantastic training that was, to follow a project from design through construction and testing to flight and data analysis, all in under 3 years! My early post-doctoral work used rocket-borne X-ray detectors - also projects that could be completed in several years. It is hard to overemphasize the importance of all this hands-on training. When I worked on proposals for satellite projects, I drew on extensive knowledge of how instruments work and data is analyzed and also how projects are conceived and executed. I had the "big picture" of a few small projects, which made it much easier to appreciate and contribute to many larger projects. What happens today to a graduate student coming into a research group in space astronomy? Working on the design of a satellite experiment is incompatible with the time constraints of students. I have seen many students get involved in data analysis of ongoing satellite programs. This has produced a great number of computer-wise students and a lot of excellent science. One has to ask, however, who is going to design and build future space astronomy payloads? Right now, the time lag from design to flight even of a simple Get-Away-Special (GAS) experiment, to say nothing of a more sophisticated SPARTAN payload, is too long for graduate students. This must be changed if these programs are to fulfill their potential for astronomical resaearch. One of the shuttle flights I worked on originally had a SPARTAN payload on it, and I have worked closely with the Goddard Space Flight Center SPARTAN project team. They are doing an excellent job, as are the GAS team. The problems in realizing the desired flight frequencies of these payloads is mostly a result of the lower-than-originally anticipated shuttle flight rate. Let us hope that we have turned the corner with at least one flight a month now over the last 6-months and that the Spartan and GAS programs will be able to increase their flight opportunities correspondingly.

I hope that my testimony today has given a balanced view of the impact of the shuttle on space astronomy-covering both problems and potential for the future. Astronomy, over the past two decades, has undergone a revolution made possible in no small part by observations from space. The increasing technical capabilities of space astronomy have closely followed advances in technology and is making its corresponding impact on space astronomy. I look forward to future astronomical endeavors involving the shuttle and, looking ahead towards the end of the century, to the use of our Space Station to further the work now being done by the shuttle.

Mr. **Nelson**. Thank you, Dr. Hoffman.

A couple of comments. In your last statement, my mind's eye flashed back to Dr. Harwit's chart that showed the astronomical discoveries over the last number of decades and the fact of the huge number of discoveries that had been made in the last 5 years.

So my question to you would be, in large part, have those discoveries come as a result of the last 4 years of shuttle flying?

Mr. **Harwit**. No. The chart actually ended in 1979.

Mr. **Nelson**. I see; 1979 and previous. OK.

Mr. **Harwit**. That's right. Because one has to have a certain perspective. Things that you hear about today sometimes disappear tomorrow as oddities which you didn't recognize correctly. So there is a slight distance.

Most of these that I showed came in the 1960's and 1970's. On the other hand, the rate of discovery has kept on going up since the time that I completed that graph. We have discovered, for example, gravitational lenses, so-called, and the planetary disk that was just mentioned by Dr. Field, and several other objects—other discoveries. None of them have come from the shuttle, though.

But I think the point that Dr. Hoffman is making is still the right one; namely, that one needs small instrumentation for training the next generation of scientists.

If we phase out the rockets, as we have been doing, and the balloons, we really have to pick that group up in the shuttle, and as Mr. Thomas here has demonstrated, it is a place where students can do outstanding work and it is also a place where we develop the instruments that will then be flown on the more expensive missions where those discoveries then eventually get made.

Mr. **Nelson**. All of you are weaving a story that is fascinating and most important for us on this committee who will be trying to make the decisions of where we're going with regard to the available funds in the future.

I might point out that our colleague Jake Garn, who was here earlier, said in his comments that if we're successful with regard to a breakthrough in space medicine for research in space, that has a profound effect upon medicine here terrestrially, that as a practical matter, that will have enormous implications in us being able to demonstrate to our colleagues in this increasing competition for available dollars the need for America's Space Program. That will just be a dramatic help to us.

Before we go to Mr. Thomas, do any of the committee members want to share any comments or questions with any of the panel? We have one further witness, the student, Mr. Thomas from Utah State.

Mr. **Lujan**. I just wanted to find out because of the trip that we're going to do in the cooperation and all that, with the Soviets now saying, you know, we will go our own—I am particularly referring to Quasat—now that they've said, "We're going to go on our own," what does that do in the area of cooperation as far as we're concerned? Does that mean we won't, or it's still open, or what?

Mr. **Harwit**. Well, I think that one of the areas that might possibly be explored would be in the realm of cosmic ray detection; that is the detection of energetic particles that come in from space.

Many of these projects require extremely massive detectors, although perhaps in some cases less sophisticated electronics.

Now, the Soviets have always had powerful boosters, so that putting up massive experiments is not a very big problem for them, not as much of a problem as sophisticated electronics and sophisticated optics, and it may very well be that there could be some collaborative effort that could be set up there.

I am guessing to some extent. It's not my immediate area of specialization. But that may, in fact, be one way that one could be doing a collaboration.

Dr. FIELD. Mr. Lujan, if I could comment. I have had the benefit of discussions with Professor Burke, who has been involved in these discussions on the international front, particularly with the Soviets very recently. The history of this is long and complicated. As I mentioned earlier to the committee, the Astronomy Survey Committee, in fact, recommended with considerable priority that a space VLBI experiment be carried out. At that time, it was not realized that there would be the potential for international collaboration, which did develop subsequently, and I think all of us were very pleased to see that happening.

The reason is simple: that we're talking about terminals in space, and the more there are, the better. And it's just a natural for international collaboration, particularly with the Soviets, who are experts in this field and who have been collaborating for a long time with their American counterparts in the field of VLBI.

So it was distressing, to say the least, to learn that only a few weeks ago the Soviets had apparently changed their original plans to share equally in a facility of this type and instead to build what appears to be a more or less self-sufficient facility in which they have invited our participation rather than more or less an equal sharing.

My own judgment—it's too early to say, really—but I think I would suggest caution. We should not overreact at this point. If any of you are, in fact, going to the Soviet Union and will be discussing these questions, I would suggest vigilance and concern and factfinding, but not to overreact at this time.

I believe that as time goes on, it should be possible to work out some effective collaborative scheme with the Soviets on Quasat.

Mr. LUJAN. Even if they move on ahead with their program, we can still hook onto it at a later time because they will have those satellites in there. Is that basically what you're saying, even if——

Dr. FIELD. Well, yes.

Mr. LUJAN. Even if right at the beginning we're not in on it with them?

Dr. FIELD. That's right. You see, it's complicated because our technology in this field is more advanced than theirs, and it was always contemplated that this would proceed in a stepwise fashion in a collaboration with them, and as the technology—or as our aspirations increased, the technology that we have, the more advanced technology, could be brought to bear in a phased fashion.

The concern at the moment in the United States is twofold, I think, that the Quasat experiment, which is self-contained that the Russians are proposing, is not optimal because it doesn't include the high technology that we would be able to put into it. As I say, it's the Soviet technology.

Second, of course, there is concern that this would be perceived in the United States as preempting the field, and that's an instance of what we were hearing about from Dr. Lew Allen yesterday of rapidly increasing sophistication of the Soviet scientific community, I think, along with the political community, in using public relations techniques that are so familiar to us in the West.

Mr. Lujan. Two real quick questions, Mr. Chairman. I don't want to take a lot of time.

Is the reason they moved ahead with their own program because of our inability to get along with VLBI? Is that it?

Dr. Field. No, I don't think so, sir. I believe that they have placed an option on the table that has been discussed at length, and after waiting a significant length of time, and not finding a strong commitment from the United States side on this, they chose to move ahead with their own project.

Mr. Lujan. Why didn't we?

Dr. Field. Well, I can't give you a very strong detailed answer, but I believe it comes down to the fact that in the NASA program at the present time, Quasat is of somewhat lower priority than some of the other projects.

Mr. Lujan. My final question. Could we work with the Soviets with the capabilities we have now with the VLA if we didn't have VLBA?

Dr. Field. Oh, no question about it. The VLA itself is an extremely powerful terminal of a VLBI instrument of any kind. And I believe that it is planned to be incorporated into any space VLBI. If the Soviets would agree to that, I don't see any reason why that couldn't be done.

Mr. Lujan. Thank you, Mr. Chairman.

Mr. Nelson. May I invite any of you all after the hearing to share with our staff any specific item that you would like us to discuss with the Soviets, and we will be happy to take your ideas to them.

All right, Mr. Thomas, you have been waiting very patiently. We appreciate your being here with us to share with us.

STATEMENT OF SCOT THOMAS, STUDENT, UTAH STATE UNIVERSITY, BRIGHAM CITY, UT

Mr. Thomas. Thank you, Mr. Nelson. Thank you for inviting me here today.

I will be bringing you the perspective of a small user of the space shuttle rather than these large users with large telescopes. I have been doing experiments and studying fluid processes in microgravity for the last few years. I have had three experiments flown on the shuttle, one in the middeck and two in getaway specials. I will be talking from experience that I have had with those with the NASA system.

I guess the basic message that I would like to bring to you today is that for lots of areas of space research I think smaller is better, and I will tell you why, so keep that in mind. And I will be talking mostly from the perspective of materials processing, but I think what I have to say applies to lots of areas of space research.

Now, experiments done in orbit are inherently more expensive, of course, than experiments done on the ground because of air safety issues, and the experiments have to be integrated into a package that is controlled by computers and things have to be automated to some degree. They're not just spread out on a lab bench with wires between them. So they will always be inherently more expensive than ground experiments.

But I think something that has driven the cost up of space experiments significantly in the past and continues to is the infrequent flight opportunities. In the past, of course, there haven't been that many flights before the shuttle program. Now there are frequent—there is frequent access to orbit, although I am not quite sure how frequent the scientific access to orbit is. It's still hard to get things on the shuttle and get them flown.

But infrequent flights lead to long leadtimes, and they also cause scientists to make the experiments very complicated. If you know you're only going to get one or two chances in a career, for example, to study something, you want to build an experiment that will study absolutely everything you could think of. So that makes the experiment very complicated and very expensive.

The other thing is you are not really sure of the methods of your experiment in zero gravity. On my middeck experiment, something as simple as getting oil into a little aluminum pan didn't work the way we thought it would, and we had to have the oil in the pan in a certain way. And we're so unfamiliar with the environment in orbit in a spacecraft that sometimes you're not even sure how to do the experiment. So that makes you spend a lot of time and effort deciding on the methods.

We put a lot of effort into trying to figure out how to get oil into a pan, but I guess we didn't put enough effort into it, whereas if you're doing an experiment on the ground, you would just put some oil in the pan and that would be that.

The infrequent flights lead experimenters to make complicated experiments, and there is a lot of effort put in. With the long leadtimes, things get expensive.

Now, why is that bad? That is bad because that's not the way science is done. Science is an iterative process. One goes into the laboratory and does an experiment, and he looks at his data and decides that a certain thing is interesting and he goes on from there and studies that. And then he decides that to find out more about his process he needs to study a certain aspect of it. And that's how science is done. It's not a one-shot thing. It's an iterative process.

If you will only get to fly an experiment once, you're not going to know everything to look for even though you spend 5 or 8 years designing and putting every kind of probe that you can think of, you still don't know exactly what might be the most interesting thing in what you're studying.

So that's the main reason why infrequent flights are bad. That's not the way science should be done.

Mr. **Nelson**. Well, you're going to share in the dramatic testing from the shuttle program and the space station.

Mr. **Thomas.** Yes. Now, for the future, with the shuttle and the space station, there will hopefully be more frequent access to space, and emphasis could, and I think should, be shifted from large, single, very expensive and complicated experiments to a large number of smaller, simpler experiments, and you will end up getting more science for the dollar that way.

At present, the way space research is done on the shuttle is in Getaway Specials. That's designed to be cheap and inexpensive and quick turnaround. And also through NASA's industry involvement, joint endeavor program—and the two obvious examples are McDonnell Douglas with their electrophoresis. That's a good example because they've flown that a lot of times and they learn things each time. If they had to wait for a flight every 5 years, they would never know whether they're going to be able to cure any diseases; they would never get any answers.

The other example is 3M. They're going to fly, with rapid turnaround, very simple experiments to see what's interesting. Now, in the future, I would hope that the space station—and I forgot to mention that spacelab is, of course, the major thrust of space research, and that's still in the mode of large, single experiments, very expensive, with long lead times. If we had only spacelab experiments, for example, I think we would never find out whether we're going to grow crystals and turn that into a billion-dollar industry or something, because we're 5 years between experiments and that's still the way it is with spacelab. There are disciplinary flights, and there are still long lead times and infrequent flights. You just can't learn things fast enough on that time scale.

So I would hope, in the future, that we could have more access for simple experiments on the space shuttle, and also don't let the space station turn into a big spacelab. Turn it into a real laboratory and make sure there is frequent access of equipment up and down and shift the emphasis to smaller experiments that are more simple.

I have just got two other comments I would like to make. I think having a man in the loop, as it's said, is very important. I think anybody who has ever had a manned space experiment with a man actually operating it would agree.

My Getaway Special experiments are self-contained, and they're controlled by a computer, so you have to think out everything that might happen and build that into the computer in the control of the experiment beforehand.

Now, we left a switch in a wrong position. It should have been down, and it was up when it flew, and that ruined the whole experiment. Something as simple as that. Now, if that had been in the middeck, we could have just said, "Hey, flip the switch the other way," and it would have been fine. So I think having a man there to actually perform experiments is very important.

The other thing is that I think many people in different disciplines will find that they can't quite tolerate the g level that will be associated with a manned space station because astronauts are jumping around and motors are running and things are vibrating a little bit. That's just inherent in a large manned vehicle. And especially for materials processing, of course, and astronomy, a large telescope couldn't accept and be mechanically coupled to a space station, but you still like to have that man there to be able to do things.

So I think a multidisciplinary free-flyer associated with the manned space station would be very useful so that certain experiments could be put there at a very low gravity level. I think 10 to the minus 3 or 10 to the minus 5 might make a big difference. So I think that's also something that should be pursued.

Mr. NELSON. Thank you, Mr. Thomas.

Dr. HOFFMAN. Mr. Nelson, I wonder if I could add something to what Mr. Thomas has said regarding the inclusion of these small-scale experiments on shuttle flights, since I have been involved in carrying a few of them out. You will have an opportunity to see how this affects the crew's activity.

Of course, on any shuttle flight we have, by and large, the major aspects of the major purposes of the mission, which for most shuttle flights is the launch of several satellites. I referred before to the problems of shuttle flight duration, but I would like to say that even without increasing the total potential duration of shuttle flights, most of our flights don't stay up as long as the shuttle could stay up.

You will find down in Houston that some crew members and also flight trainers, people responsible for the manifesting, are sometimes somewhat resistive of adding a lot of things onto the shuttle flight manifest because of the crew workload, the inherent limitations on time available to carry things out.

But I think if you asked any crew member in the program, "If we were to give you an extra day in orbit, would you be willing to do the extra work to carry out six different middeck experiments," you'd get 100-percent agreement.

Of course, the total, although it does cost extra money to keep the shuttle up for each additional day, the incremental costs compared to what it took to get up there in the first place has got to be very small.

So I would hope that the direction we're moving in is that every time the shuttle goes up, given the opportunity for weight and stowage—and by the way, stowage is one of the big problems we have. If we had more stowage onboard the shuttle, we could do an awful lot more of the small middeck-oriented science. And that may be one of the improvements that we will have to buy in the future.

Anyway, this is a relatively inexpensive way to increase the use of the shuttle for valuable space science.

Mr. NELSON. Very good. Thank you.

One of you made an earlier comment, and then we didn't explore it—it might have been you, Dr. Field—that the theory is that comets and asteroids have had a direct effect upon the evolution of the Earth as we know it today. Do you want to expand on that?

Dr. FIELD. Yes, I would be happy to, Mr. Nelson.

The first has to do with where carbon came from on the Earth. I have done some reading on this subject, and my belief is that carbon may have arrived on the Earth in the form of cometary materials which were brought along by comets in the bombardment that succeeded the formation of the Earth.

Carbon is pretty important because all living things are essentially dependent upon it, and so it could be that as we get further into cometary research, this will be either validated or invalidated, that life on Earth depends upon comets.

Life on Earth is also from time to time apparently interrupted by the collision of bodies with the Earth, we think approximately every 65 million years or so, and that these bodies have a potentially disastrous effect because of the dust that is thrown up by the impact, which is retained by the gravitation field of the Earth in the atmosphere, giving you a dust pall over the whole globe, which can cut off sunlight and thereby interrupt the food chain and perhaps lead to a disaster.

I think it's correct, however, that we are about 30 million years since the last such event, something like that. We don't expect one to happen momentarily.

Mr. **Nelson**. Our chances are pretty good?

Dr. **Field**. Our chances are a little better than average that we will get through the next few years.

But it is an interesting phenomenon to explore for the fundamental science involved.

Mr. **Nelson**. Well, that's interesting.

Now, I would assume that you support comet rendezvous missions suggested for the nineties.

Dr. **Field**. I do. I think that the planetary exploration program that has been laid out by SSEC is a very strong one, and I personally support it. I happen to have a strong interest in planetary research, and particularly agree very strongly with the comment made by that community, and I think by David Morrison, that the comets and small bodies of the solar system may prove to be the most interesting of all because of some of these connections that I have mentioned. Therefore, I do strongly support CRAF.

Mr. **Nelson**. Are we going to get a good bit of data from the Soviet and European mission, the Halley's comet, of which we have a couple of instruments onboard the Soviet Vega spacecraft and our deep-space network is being used with?

Dr. **Field**. Yes, I believe so, Mr. Nelson. As you know, this is an area where there is an international committee working to coordinate all of these efforts. I believe it will be a successful venture.

However, I personally have some regret that a truly extremely imaginative and exciting mission that had been proposed by JPL back in the seventies to Halley's comet involving a solar sail, if you remember, a fantastic new way of propulsion in the solar system, was not approved at that time, because I think it would have opened up an extraordinary opportunity to rendezvous with the comet.

As you know, the fly-bys that we're going to see in 1986 are all extremely high-velocity fly-bys where the time available for experiments is very, very short. The solar sail mission that was proposed by JPL would have enabled us to rendezvous with it and to spend months, even years, I believe, in the neighborhood of the comet itself, which would have been an absolutely breakthrough kind of thing if we had been able to do it.

Mr. **Lujan**. Mr. Chairman, I have one other comment. One of our other subcommittees we deal with, with NSF and, of course, you know they operate the ground-based astronomy, when we get all these satellites up there, are we going to shut down the ground-based facilities? What do you see?

Mr. **Field**. Well, Mr. Lujan, I hope that we don't react in a knee-jerk fashion in that way. As you know, the history of the subject has been that at least the premier instruments of the previous generation continue to be used long after other instruments have become perhaps the forerunners.

As an example, one thinks of the 100-inch telescope on Mt. Wilson. I believe it was established early in this century, maybe 1905, 1910, something like that. It has been used continuously up until 1985. Only now is it being considered for possible closure, 80 years or so of use, even though during some 30 or 40 of those years it was outranked by a large number of other telescopes.

The fact is that astronomy is growing rapidly. It continues to be fascinating. It continues to make discoveries, as Dr. Harwit showed. And in retrospect, I think we would find it foolish to, without very careful thought, close down facilities that are still producing good data.

I realize the budgetary pressures. I would only counsel that before doing something like that, that there be a careful study to examine the tradeoffs.

Mr. **Harwit**. Could I add to that perhaps that there has been some concern actually in precisely the opposite direction; namely, that with the Hubble space telescope going up and also the gamma ray observatory and, we hope very soon thereafter, the AXAF, the x-ray astrophysics facility, that there will be an enormous amount of ground-based correlative searching that will have to be done; that is that we will be uncovering new data, new discoveries at a very fast pace, that the data rate, the rate at which information is going to be arriving will greatly increase.

For example, just for the Hubble space telescope alone, it is certainly going to exceed that of all other observatories combined, ground-based and in space, when it's first launched. So with all that information flow coming in, there will be an impetus for following up these discoveries with normal ground-based observatories which have an enormous flexibility in instrumentation that can be put on.

The Hubble space telescope, when it goes up, will have a fixed complement of instruments and will be limited, of course, although with enormous capabilities, nevertheless limited in the types of things that it can do.

You will want to follow up with ground-based observations, and one of the questions which the Astrophysics Council, a body at NASA that I chair, is planning to put on its agenda is the question of how that ground-based observing ought to be supported because it looks as though it's going to be far beyond the current NSF capabilities and yet traditionally there has been a division between NSF and NASA in that NSF has done most of the ground-based observations, and here now NASA will be stimulating a rash of observing which is far beyond anything which in the past has been supported. This is a problem then that we will have to somehow untangle in the next few years.

Mr. **Lujan**. The only concern—if I might just comment about NASA doing all of the observations and taking charge of the full program is that we have felt, many of us, that NASA is top-heavy in operation now—maybe "top-heavy" is not the—but very involved in operation now, and there is a concern that research kind of takes a back seat anytime you have a project. It's not just NASA that research takes a back seat.

Mr. **Harwit**. But that's an unfortunate thing, Mr. Lujan. I guess we talked about it here a little bit. It's something that one has to try to work out at a human level, I suppose, to try to arrive at a sensible relationship in spite of or in concert with the machinery which is available to one.

Mr. **Nelson**. OK. Let me ask one further question. Dr. Hoffman, since the Einstein X-ray Observatory went out, and until the time that we put AXAF up, that's probably a period of around 12 years. So the question is, what does that do to the x-ray astronomy community?

Dr. **Hoffman**. I think what has happened is that the focus of x-ray astronomy has shifted from the United States to other parts of the world. As Dr. Field mentioned, this was something of which we have great cause to feel pride of having opened up this field in this country, and I think it's been little short of tragic that we have lost this tremendous observational capability.

I mean the impact on active x-ray astronomy as done here in the United States I think is comparable to what would happen to optical astronomy if this year we should close down all optical observatories. When American x-ray astronomers want to get new data, they basically have to collaborate with the Europeans or the Japanese.

Mr. **Nelson**. Do you think the AXAF technology is proceeding okay?

Dr. **Hoffman**. AXAF has been postponed from year to year to year. As you say, there is this 12-year gap which is essentially intolerable from the point of view of maintaining the observational expertise. People have to go elsewhere.

In fact, the impact on a good number of observational x-ray astronomy groups in this country has been perhaps—well, I don't think "catastrophic" is too strong a word. There has been a withering away of a great deal of the expertise and the ability to construct space experimentation in groups that were once strong and thriving, and it's been the result of not having the opportunity to fly space experiments.

Perhaps had the opportunity to fly some smaller satellites been made available or to fly some experiments on the shuttle been made available at an earlier date, we would have avoided the loss of these capabilities.

Mr. **Nelson**. Dr. Field.

Dr. **Field**. Yes, I had a brief point to make there, Mr. Nelson, that I happened to be at the Center for Astrophysics, where much of the work on the Einstein Observatory was done and where there is still a group of experts who are working on AXAF in consultation with a group at Marshall Space Flight Center, and I did have the opportunity to discuss with that group very recently where the mirror technology is proceeding in an orderly fashion.

My understanding is that it is and that an increment of funding which would continue that work, although modest on the scale of the whole program, would be extremely important in trying to get an appropriate new start for AXAF.

Now, I don't consider myself an expert in this area, but I do suggest that perhaps you or your staff talk to a Dr. Leon von Speybrock, who is at the Center for Astrophysics, and he is considered the ranking expert on x-ray mirror technology. He is a world expert, and he's heavily involved in the program, and he is also a very honest person.

Mr. NELSON. Dr. Harwit, I am going to have to keep you to a minute because we are down now to less than ten minutes.

Mr. HARWIT. OK. Yes. The mirror technology and pursuing that is a really an extremely important item. I think if one wants to avoid cost overruns in the long run, to provide these complex observatories with opportunities to tackle the most difficult technological problems in an orderly fashion, we have been depending in the past on new starts as a signal to have everyone racing off and at the same time also immediately bringing in a fairly heavy component of industrial support.

There are stages in a program like AXAF where key scientific and technological problems have to be solved. There should be ways of providing funding levels at the $10 to 20 million level for solving the AXAF mirror problem well before one starts bringing in the standing army which always costs a great deal. I think that's the issue that one is really addressing here.

Mr. NELSON. I thank all of you. It has been a most stimulating morning.

Dr. FIELD. Thank you, Mr. Nelson, for your attention.

Mr. NELSON. You are very kind.

The meeting is adjourned. I am going to dash off to vote, but we will be having a press conference right here prior to our departure to Russia this evening, so if any of you all would like to stay and participate in that, you're welcome.

Thank you.

[Whereupon, at 12:04 p.m., the subcommittee was adjourned.]

APPENDIX

The University of Iowa
Iowa City, Iowa 52242

Department of Physics and Astronomy
(319) 353-4343

4 November 1985

The Honorable Don Fuqua, Chairman
Committee on Science and Technology
2321 Rayburn House Office Building
U.S. House of Representatives
Washington, DC 20515

Attn: Subcommittee on Space Science and Applications

Dear Mr. Chairman:

1. Returned herewith is my annotated copy of the transcript of 8 October 1985 hearings of the Subcommittee on Space Science and Applications in which I participated as a witness. I have made, in red ink, a few minor corrections in accordance with your guidelines, but have not attempted to correct my numerous errors of syntax and grammar.

2. I request that the following information be included in the hearing record:

Page	Line(s)	Comment
37	803, 804	As it stands, this is an obvious misstatement because of my inadvertent use of the term "Gross National Product" rather than "gross NASA budget". These two lines should be replaced by "will find that the implied costs are a substantial fraction of the present NASA budget".
72	1584	The figure 1,058 should be replaced by 1,037.
72	1598	The figure 1,058 should be replaced by 1,037.

I understand that this letter may be inserted in the transcript in the form of an appendix and referred to by footnote in the main text.

Sincerely yours,

J.A. Van Allen
James A. Van Allen

NASA
National Aeronautics and
Space Administration
Washington, D.C.
20546

NOV 8 1985

Reply to Attn of: C:DBD:C17231

Honorable Don Fuqua
Chairman
Committee on Science
  and Technology
House of Representatives
Washington, D.C. 20515

Dear Mr. Chairman:

It is requested that the following correction be included in the hearing record resulting from the October 9, 1985, hearing at which Dr. Allen testified before the Subcommittee on Space Science and Applications regarding the solar system exploration program.

Page	Line(s)	Proposed Wording
67	1543-1547	The rings and the moons and the atmosphere of Uranus are bound to be different than any that we have seen before, and so this encounter is going to be a different one than Jupiter and Saturn but is bound to also be a very exciting encounter.
68	1555-1560	That will enter into orbit about Jupiter, with its first encounter very close to the surface of Io, one of the major moons of Jupiter. Galileo will also deploy a probe that will descend into the atmosphere until that probe is destroyed by the heat and pressure of the Jovian atmosphere.

We understand that this letter may be inserted in the transcript in the form of an appendix and referred to by footnote in the main text.

Sincerely,

Lynn W. Heninger
Acting, Director
  Congressional Liaison Division

Center for Astrophysics
60 Garden Street
Cambridge, Massachusetts 02138

Harvard College Observatory
Smithsonian Astrophysical Observatory

October 25, 1985

The Honorable Don Fuqua, Chairman
Committee on Science and Technology
2321 Rayburn House Office Building
U.S. House of Representatives
Washington, D.C. 20515

Attention: Subcommittee on Space Science and Applications

Dear Mr. Chairman:

As a witness before the Subcommittee on Space Science and Applications on October 10, 1985, I have edited and am returning the hearing transcript forwarded to me for review. I am returning the transcript, and I request that the following information be included in the hearing record:

Page	Line	Proposed Wording
7	144	Add "study" - otherwise meaningless
8	152	Suomi
10	208	"and" makes the sentence correct.
11	226	Makes little sense as written.
11	228	Negative omitted.
12	258	I inadvertently skipped part of my presentation and so went back and picked it up.
13	296	Transcription error.
20	450	Transcription error.
20	452	"or" added for clarity.
20	455	misspelling - "VLBI".
20	463	Solar Optical Telescope a project, hence capitalized.
20	480	My error: "Future" should be "Major".
24	549-551	Clarify a scientific explanation.

Page	Line	Proposed Wording
24	554-555	Clarify a scientific explanation.
24	557	Clarify a scientific explanation.
24	566	Clarify a scientific explanation.
28	647	Clarify a scientific explanation.
28	663/4	Clarify a scientific explanation.
29	670	I want to distinguish planetary scientists from astronomers in general.
72	1702	VLBI
73	1727	Quasat is a VLBI project.
74	1751	Clarification: remove "increased".
74	1758	I believe Mr. Lujan may mean "VLBA", a ground-based project with which he is familiar. At least that is the basis upon which I responded.
74	1773	Here, "VLBA" was incorrectly transcribed as "VLVA", I believe.
74	1775	"VLV" → "VLBI".
86	2048	Doesn't make sense as written.

I understand that this letter may be inserted in the transcript in the form of an appendix and referred to by footnote in the main text.

Sincerely,

George Field

George Field

GF/dar

Enclosure